NEW SERIES. No. 27.] [JULY, 1898.

MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY.

I.—THE ESSENCE OF REVENGE.[1]

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IN his remarkable work, *Ethnologische Studien zur ersten Entwicklung der Strafe*, Dr. S. R. Steinmetz has made the feeling of revenge the object of an investigation which seems to deserve more attention than has hitherto been bestowed upon it by psychologists. Not only do his *Studien* contain one of the very few attempts ever made by a specialist to elucidate a general psychical phenomenon by means of ethnological facts; but the problem he discusses is of wide-extending importance, bearing, as it does, not only directly upon the question of revenge, but, also, indirectly upon the explanation of the most prominent element in the moral consciousness. It will be the object of the present article to take up this problem afresh, not indeed to give anything like a general psychology of revenge, still less to deal with it as a social phenomenon, but to survey what the available facts really seem to teach us regarding its essence.

The ultimate conclusions at which Dr. Steinmetz has arrived are these: Revenge is essentially rooted in the feeling of power and superiority. It arises consequently upon the experience of injury, and its aim is to enhance the "self-feeling" which has been lowered or degraded by the injury suffered. It answers this purpose best if it is directed against the aggressor himself, but it is not essential to it that it should take any determinate direction, for, *per se* and originally, it is "undirected" and unlimited.

Strictly speaking, this theory is not new. At least Dr.

[1] Read before the Aristotelian Society.

Paul Rée, in his book, *Die Entstehung des Gewissens*, has pronounced revenge to be a reaction against the feeling of inferiority which the aggressor impresses upon his victim. The injured man, he says, is naturally reluctant to feel himself inferior to another man, and consequently strives, by avenging the aggression, to show himself equal or even superior to the aggressor.[1] But Dr. Steinmetz has elaborated this theory with an independence and a fulness which make any question of priority quite insignificant.

The first stage, he says, through which revenge passed within the human race was characterised by a total, or almost total, want of discrimination. The aim of the offended man was merely to raise his injured "self-feeling" by inflicting pain upon somebody else, and his savage desire was satisfied whether the man on whom he wreaked his wrath was guilty or innocent.[2] No doubt, there were from the outset instances in which the offender himself was purposely made the victim, especially if he was a fellow-tribesman; but it was not really due to the feeling of revenge if the punishment was inflicted upon him, in preference to others. Even primitive man must have found out that vengeance directed against the actual culprit, besides being a strong deterrent to others, was a capital means of making a dangerous person harmless. However, Dr. Steinmetz adds, these advantages should not be overestimated, as even the indiscriminate revenge has a deterring influence on the malefactor.[3] In early times, then, vengeance, according to Dr. Steinmetz, was in the main "undirected".

At the next stage it becomes, he says, somewhat less indiscriminate. A proper victim is sought for even in cases of what we should call natural death, which the savage generally attributes to the ill-will of some foe skilled in sorcery;[4] though indeed Dr. Steinmetz doubts whether in such cases the unfortunate sufferer is really supposed to have committed the deed imputed to him.[5] At all events, a need is felt of choosing somebody for a victim, and "undirected" vengeance gradually gives way to "directed" vengeance. A rude specimen of this is the blood-feud, in which the individual culprit is left out of consideration, but war is carried on against the group of which he is a member, either his

[1] Rée, *Die Entstehung des Gewissens*, § 14, p. 40.

[2] Steinmetz, *Ethnologische Studien zur ersten Entwicklung der Strafe* (Leiden, 1894), vol. i., pp. 355, 356, 359, 361.

[3] *Ibid.*, vol. i., p. 362.

[4] *Ibid.*, vol. i., p. 356 *sq.*

[5] *Ibid.*, vol. i., p. 359 *sq.*

family or his tribe. And from this system of joint responsibility we finally come, by slow degrees, says Dr. Steinmetz, to the modern conception, according to which punishment should be inflicted upon the criminal and nobody else.[1] Dr. Steinmetz believes that the *vis agens* in this long process of evolution lies in the intellectual development of the human race: man found out more and more distinctly that the best means of restraining wrongs was to punish a certain person, *viz.*, the wrong-doer.[2] On this utilitarian calculation our author lays much stress in the latter part of his investigation; whereas in another place he observes that a revenge which is directed against the offender is particularly apt to remove the feeling of inferiority, by effectually humiliating the hitherto triumphant foe.[3]

In this historical account the main points of interest are the initial stage of "undirected" vengeance, and the way in which such vengeance gradually became discriminate. If, in primitive times, a man did not care in the least on whom he retaliated an injury, then of course the direction of his vengeance could not be essential to the revenge itself, but would be merely a later appendix to it. Now the question is, what evidence can Dr. Steinmetz adduce to support his theory? Of primitive man we have no direct experience; no savage people now existing is a faithful representative of him, either physically or mentally. Yet however greatly the human race has changed, primitive man is not altogether dead. Traits of his character still linger in his descendants; and of primitive revenge, we are told, there are sufficient survivals left.[4]

Under the heading "Perfectly Undirected Revenge," Dr. Steinmetz sets out several alleged cases of such so-called survivals.[5] 1. An Indian of the Omaha tribe, who was kicked out of a trading establishment which he had been forbidden to enter, declared in a rage that he would revenge himself for an injury so gross, and, "seeking some object to destroy, he encountered a sow and pigs, and appeased his rage by putting them all to death". 2. The people of that same tribe believe that if a man who has been struck by lightning is not buried in the proper way, and in the place where he has been killed, his spirit will not rest in peace, but will walk about till another person is slain by lightning and laid beside him. 3. At the burial of a Loucheux Indian, the

[1] Steinmetz, *Ethnologische Studien zur ersten Entwicklung der Strafe* (Leiden, 1894), vol. i., p. 361.

[2] *Ibid.*, vol. i., pp. 358, 359, 361 *sq.*

[3] *Ibid.*, vol. i., p. 111.

[4] *Ibid.*, vol. i., p. 364.

[5] *Ibid.*, vol. i., p. 318 *sqq.*

relatives sometimes will cut and lacerate their bodies, or, as sometimes happens, will, "in a fit of revenge against fate," stab some poor, friendless person who may be sojourning among them. 4. The Navajoes, when jealous of their wives, are apt to wreak their spleen and ill-will upon the first person whom they chance to meet. 5. The Great Eskimos, as it is reported, once after a severe epidemic, swore to kill all white people who might venture into their country. 6. The Australian father, whose little child happens to hurt itself, attacks his innocent neighbours, believing that he thus distributes the pain among them and consequently lessens the suffering of the child. 7. The Brazilian Tupis ate the vermin which molested them, for the sake of revenge; and if one of them struck his foot against a stone, he raged over it and bit it, whilst, if he were wounded with an arrow, he plucked it out and gnawed the shaft. 8. The Dacotahs avenge theft by stealing the property of the thief or of somebody else. 9. Among the Tshatrali (Pamir), if a man is robbed of his meat by a neighbour's dog, he will, in a fit of rage, not only kill the offending dog, but will, in addition, kick his own. 10. In New Guinea the bearers of evil tidings sometimes get knocked on the head during the first outburst of indignation evoked by their news. 11. Some natives of Motu, who had rescued two shipwrecked crews and safely brought them to their home in Port Moresby, were attacked there by the very friends of those they had saved, the reason for this being that the Port Moresby people were angry at the loss of the canoes, and could not bear that the Motuans were happy while they themselves were in trouble. 12. Another story from New Guinea tells us of a man who killed some innocent persons, because he had been disappointed in his plans and deprived of valuable property. 13. Among the Maoris it sometimes happened that the friends of a murdered man killed the first man who came in their way, whether enemy or friend. 14. Among the same people, chiefs who had suffered some loss often used to rob their subjects of property in order to make good the damage. 15. If the son of a Maori is hurt, his maternal relatives, to whose tribe he is considered to belong, come to pillage his father's house or village. 16. If a tree falls on a Kuki his fellows chop it up, and if one of that tribe kills himself by falling from a tree the tree from which he fell is promptly cut down. 17. In some parts of Daghestan, when the cause of a death is unknown, the relatives of the deceased declare some person chosen at random to have murdered him, and retaliate his death upon that person.

I have been obliged to enumerate all these cases for the reason that a theory cannot be satisfactorily refuted unless on its own ground. I may confess at once that I scarcely ever saw an hypothesis vindicated by the aid of more futile evidence. The cases 7 and 16 illustrate just the reverse of "undirected" revenge, and, when we take into consideration the animistic belief of savages, present little to astonish us. In case 17 the guilt is certainly imputed to somebody at random, but only when the culprit is unknown. Cases 1, 4, 10 and 12 and perhaps also 11, imply that revenge is taken upon an innocent party in a fit of passion;—in cases 1 and 12 the offender himself cannot be got at, in case 10 the man who is knocked on the head appears for the moment as the immediate cause of the grief or indignation evoked, while case 11 exhibits envy combined with extreme ingratitude. In case 9 the anger is chiefly directed against the "guilty" dog, and against the "innocent" one evidently by an association of ideas. Cases 8 and 14 illustrate indemnification for loss of property, and in case 8 the thief himself is specifically mentioned first. In case 15 the revenging attack is made upon the property of those people among whom the child lives, and who may be considered responsible for the loss its maternal clan sustains by the injury. Case 6 merely shows the attempt of a superstitious father to lessen the suffering of his child. As regards case 5, Petitot, who has recorded it, says expressly that the white people were supposed to have caused the epidemic by displeasing the god Tornrark.[1] Case 2 points to a superstitious belief which is interesting enough in itself, but which, so far as I can see, is without any bearing whatever on the point we are discussing. Case 3 looks like a death-offering. The stabbing of an innocent person is mentioned in connexion with, or rather as an alternative to, the self-laceration of the mourners, which last undoubtedly has a sacrificial character. Moreover, there is in this case no question of a culprit. In case 13, finally, the idea of sacrifice is very conspicuous. Dr. Steinmetz has borrowed his statement from Waitz, whose account is incomplete. Dieffenbach, the original authority, says that the custom in question was called by the Maori *taua tapu*, *i.e.*, sacred fight, or *taua toto*, *i.e.*, fight for blood. He describes it as follows: "If blood has been shed, a party sally forth and kill the first person they fall in with, whether an enemy or belonging to their own tribe; even a brother is sacrificed. If they do not fall

[1] Petitot, *Les Grands Esquimaux*, p. 207 *sq.*

in with anybody, the *tohunga* (that is, the priest) pulls up some grass, throws it into a river, and repeats some incantation. After this ceremony, the killing of a bird, or any living thing that comes in their way, is regarded as sufficient, provided that blood is actually shed. All who participate in such an excursion are *tapu*, and are not allowed either to smoke or to eat anything but indigenous food."[1]

There can be no doubt that this ceremony was undertaken in order to appease the enraged spirit of the dead.[2] The question, however, is, why was not his death avenged upon the actual culprit? To this Dr. Steinmetz would answer that the deceased was thought to be indiscriminate in his craving for vengeance.[3] The "sacred fight" of the Maori, however, only seems to illustrate the impulsive character of anger in connexion with a superstitious belief. From Dieffenbach's description of it, it is obvious that the relatives of the slain man considered it to be a matter of paramount importance that blood should be shed immediately. If no human being came in their way, an animal was killed. This, I think, we may explain without difficulty, if we consider the terror which the supposed wrath of the dead man's spirit undoubtedly struck into the living. The Maoris, according to the Rev. R. Taylor, considered all spirits of the dead to be maliciously inclined towards them,[4] and the ghost of a person who had died a violent death was certainly looked upon as especially dangerous. The craving for instantaneous expiation is even more conspicuous in another case which may be appropriately mentioned in this connexion. The Aëtas of the Philippine Islands, we are told, "do not always wait for the death of the afflicted before they bury him. Immediately after the body has been deposited in the grave, it becomes necessary, according to their usages, that his death should be avenged. The hunters of the tribe go out with their lances and arrows to kill the first living creature they meet with, whether a man, a stag, a wild hog, or a buffalo."[5]

Dr. Steinmetz himself quotes, in support of his theory, some other instances from the same group of islands, in which, when a man dies, his nearest kinsmen go out to requite his death by the death of the first man who comes in

[1] Dieffenbach, *Travels in New Zealand*, vol. ii., p. 127.
[2] *Cf. ibid.*, vol. ii., p. 129.
[3] *Cf.* Steinmetz, *loc. cit.*, vol. i., p. 343.
[4] Taylor, *Te Ika a Maui* (1870), p. 221.
[5] Earl, *Papuans*, p. 132.

their way. He also refers to some statements concerning various Australian tribes, according to which the relatives of a deceased person kill some innocent man, evidently in order to appease his spirit, and perhaps, also, to a certain extent, from a feeling of revenge on their own account.[1] But all these statements prove nothing of what they are intended to prove. In every case the avenged death is "natural" according to our terminology, and caused by sorcery in the belief of the savages. Moreover, the Philippine Islanders are known to have the very worst opinion of their ghosts, who are supposed to be particularly blood-thirsty soon after death;[2] and the Australian natives very commonly tie up the limbs of the dead bodies, in order to prevent the deceased from coming out of the tomb to injure the survivors.[3]

To sum up: all the facts Dr. Steinmetz has adduced as evidence for his hypothesis of an original stage of "undirected" revenge only show, that in certain circumstances, either in a fit of passion, or when the actual offender is unknown or out of reach, revenge may be taken on an innocent being, wholly unconnected with the inflicter of the injury it is sought to revenge. Now this, as everybody knows, may happen not only among savages, but in the midst of the highest civilisation. Among ourselves it is by no means unusual that an enraged person wreaks his wrath upon people who have done him no harm whatsoever, and that an official who has been humiliated by his superior retaliates on those under him. But this can hardly be called revenge in the true sense of the word; it is sudden anger, or it is the outburst of a wounded "self-feeling," which, when not directed against its proper object, can afford only an inadequate consolation to a revengeful man. Nevertheless, although Dr. Steinmetz's facts disclose no new point in the psychology of revenge, they give us an interesting lesson with reference to another feeling, *viz.*, sympathy. Several of Dr. Steinmetz's cases record not sporadic and occasional outbursts of revengeful feeling, but established and recognised customs, and show to what an extreme the sufferings of innocent people are disregarded among many savage races.

Not only has Dr. Steinmetz failed to prove his hypothesis,

[1] Steinmetz, *loc. cit.*, p. 335 *sqq.*

[2] Blumentritt, "Der Ahnencultus und die religiösen Anschauungen der Malaien des Philippinen-Archipels," in *Mittheil. der Geogr. Gesellsch. in Wien*, vol. xxv., p. 166 *sqq.* De Mas, *Informe sobre el estado de las Islas Filipinas en* 1842, *Orijen*, etc., p. 15.

[3] Curr, *The Australian Race*, vol. i., pp. 44, 87.

but, as far as I can see, this hypothesis is quite opposed to all the most probable ideas we can form with regard to the revenge of early man. For my own part I am convinced that we may obtain a good deal of knowledge about the primitive condition of the human race, but certainly not by studying modern savages only. I have dealt with this question at some length in another place,[1] and wish now merely to point out that those general physical and psychical qualities which are not only common to all races of mankind, but which are shared by them with the animals most allied to man, may be assumed to have been present also in the earlier stages of human development. Now, concerning revenge among animals, more especially among monkeys, many anecdotes have been told by trustworthy authorities. On the authority of a zoologist "whose scrupulous accuracy was known to many persons," Mr. Darwin relates the following story: "At the Cape of Good Hope an officer had often plagued a certain baboon, and the animal, seeing him approaching one Sunday for parade, poured water into a hole and hastily made some thick mud, which he skilfully dashed over the officer as he passed by, to the amusement of many bystanders. For long afterwards the baboon rejoiced and triumphed whenever he saw his victim."[2] Prof. Romanes considers this to be a good instance of "what may be called brooding resentment deliberately preparing a satisfactory revenge".[3] This, I think, is to put into the statement somewhat more than it really contains; but at all events it records a case of revenge, in the sense in which Dr. Steinmetz uses the word. The same may be said of other instances mentioned by so accurate observers as Brehm and Rengger in their descriptions of African and American monkeys.[4] I find it inconceivable that anybody, in the face of such facts, could still believe that the revenge of early man was at first

[1] *The History of Human Marriage*, p. 3 *sqq.*

[2] Darwin, *The Descent of Man* (1890), p. 69.

[3] Romanes, *Animal Intelligence*, p. 478.

[4] Brehm, *Thierleben* (1880), vol. i., p. 156. Rengger (*Naturgeschichte der Säugethiere von Paraguay*, p. 52) gives the following information about the Cay: "Fürchet er . . . seinen Gegner, so nimmt er seine Zuflucht zur Verstellung, und sucht sich erst dann am ihm zu rächen, wenn er ihn unvermuthet überfallen kann. So hatte ich einen Cay, welcher mehrere Personen, die ihn oft auf eine grobe Art geneckt hatten, in einem Augenblicke biss, wo sie im besten Vernehmen mit ihm zu sein glaubten. Nach verübter That kletterte er schnell auf einen hohen Balken, wo man ihm nicht beikommen konnte, und grinste schadenfroh den Gegenstand seiner Rache an."

essentially undiscriminating, and became gradually discriminating merely from considerations of social expediency.

As a matter of fact, revenge only forms one link in that chain of mental phenomena, for which resentment is, perhaps, the most appropriate general name.[1] The word revenge generally implies undue severity, but when we use it as a psychological term to denote a mental state, the moral character of which appears in a very different light to different peoples, it seems advisable to strip it of all ethical qualification and to make it synonymous with deliberate resentment. It would thus represent the more intellectual form of resentment, in which the connexion between the pain inflicted and the volitional reaction is interrupted by a consideration of attendant circumstances, whereas in sudden resentment or anger the reaction takes place almost instantaneously. But it is of course impossible to draw any distinct limit between these two types, and, though brooding revenge is probably restricted to man, the cases of resentment among monkeys, quoted above, certainly indicate a certain amount of deliberation. Nor is it possible exactly to discern where an actual intention to inflict pain comes in. In its primitive form anger contains a vehement volition to remove the cause of pain, but undoubtedly it contains no real desire to produce suffering.[2] Anger is strikingly shown by many fish, and notoriously by sticklebacks when their territory is invaded by other sticklebacks. In such circumstances of provocation the whole animal changes colour, and, darting at the trespasser, shows rage and fury in every movement,[3] but, of course, we cannot believe that any idea of inflicting pain is present to its mind. As we proceed still lower down the scale of animal life, we find the volitional element itself gradually dwindle away until nothing is left but mere reflex action.

In this long chain there is no missing link. Protective reflex action, anger without intention to cause suffering, anger with such an intention, more deliberate resentment or revenge—all these phenomena are so inseparably connected with each other that no one can say where one passes into another. The common characteristic of these phenomena is this, that they are means of protection for the animal, and, if the involuntary reflex action be excluded, we may add the

[1] *Cf.* Fowler, *The Principles of Morals* (1887), vol. ii., p. 105.

[2] There are some good remarks on this in Mr. Hiram Stanley's *Studies in the Evolutionary Psychology of Feeling*, p. 138 *sq.*

[3] Romanes, *loc. cit.*, p. 246 *sq.*

further characteristic that they are mental states marked by a hostile attitude towards the cause of pain. They are useful instincts, which, like other useful instincts, have been acquired by means of natural selection in the struggle for existence.

Two different attitudes may be taken by an animal towards another which has made it feel pain: it may either shun or attack its enemy. In the former case its action is prompted by fear, in the latter by anger; and it depends on the circumstances which of these feelings is the actual determinant. Both of them are of supreme importance for the preservation of the species, and may consequently be regarded as elements in the animal's mental constitution which admit of no further explanation than that derived from their usefulness. We have already seen that the instinct of attacking the enemy could not originally have been guided by a representation of the enemy as suffering. As, however, a successful attack is necessarily accompanied by such suffering, the desire to produce it naturally, with the increase of intelligence, entered as an important element in resentment. The need for protection thus lies at the foundation of resentment in all its forms.

This view, as everybody knows, has by no means the attraction of being new. More than one hundred and fifty years before Darwin, Shaftesbury wrote of resentment in these words: "Notwithstanding its immediate aim be indeed the ill or punishment of another, yet it is plainly of the sort of those [passions] which tend to the advantage and interest of the self-system, the animal himself; and is withal in other respects contributing to the good and interest of the species".[1] A similar opinion is expressed by Butler, according to whom the reason and end for which man was made liable to anger is, that he might be better qualified to prevent and resist violence and opposition, while deliberate resentment "is to be considered as a weapon, put into our hands by nature, against injury, injustice, and cruelty".[2] Adam Smith, also, believes that resentment has "been given us by nature for defence, and for defence only," as being "the safeguard of justice and the security of innocence".[3] Exactly the same view is taken by several modern evolutionists as regards the "end" of resentment, though they, of course, do not rest contented with saying that this feeling has been given us by

[1] Shaftesbury, *An Inquiry Concerning Virtue or Merit* (1699), book ii., pt. ii., sect. ii.

[2] Butler, *Sermon VIII.—Upon Resentment.*

[3] Adam Smith, *The Theory of Moral Sentiments*, pt. ii., sect. ii., ch. i.

nature, but try to explain in what way it has developed. "Among members of the same species," says Mr. Herbert Spencer, "those individuals which have not, in any considerable degree, resented aggressions, must have ever tended to disappear, and to have left behind those which have with some effect made counter-aggressions."[1] Mr. Hiram Stanley, too, quoting Junker's statement regarding the pigmies of Africa, that "they are much feared for their revengeful spirit," observes that "other things being equal, the most revengeful are the most successful in the struggle for self-conservation and self-furtherance".[2] This evolutionist theory of revenge has been criticised by Dr. Steinmetz, but in my opinion with no success. He remarks that the *feeling* of revenge could not have been of any use to the animal, even though the *act* of vengeance might have been useful.[3] But this way of reasoning, according to which the whole mental life would be excluded from the influence of natural selection, is based on a false conception of the relation between mind and body, and, ultimately, on a wrong idea of cause and effect.

While rejecting Dr. Steinmetz's hypothesis as regards the nature of revenge, I by no means deny that a violation of the "self-feeling" is an extremely common and powerful incentive to resentment. Nothing more easily rouses in us anger and a desire for retaliation, nothing is more difficult to forgive, than an act which indicates contempt, or disregard of our feelings. Long after the bodily pain of a blow has ceased, the mental suffering caused by the insult survives and calls for vengeance. I find, however, no need to resort to different principles in order to explain the resentment excited by these different kinds of pain. In all cases revenge implies, primordially and essentially, a desire to cause pain or destruction in return for hurt suffered, whether the hurt be bodily or mental; and if, as is often the case, to this desire is added the intention to enhance the wounded "self-feeling" this does not interfere with the true nature of the primary feeling of revenge. That Dr. Steinmetz's explanation cannot be correct seems to me evident from the following facts, among others. On the one hand, we have genuine specimens of resentment without the co-operation of self-regarding pride;[4] stupidity, for instance, has a decided tendency to provoke

[1] Spencer, *The Principles of Ethics*, vol. i., p. 361 *sq.*

[2] Hiram Stanley, *loc. cit.*, p. 180. *Cf.* also Guyau, *Esquisse d'une Morale sans obligation ni sanction*, p. 162 *sq.*

[3] Steinmetz, *loc. cit.*, vol. i., p. 135.

[4] *Cf.* Bain, *The Emotions and the Will* (1880), p. 177.

anger. On the other hand, the action of self-regarding pride may be totally free from malice. If a man has written a bad book which is severely criticised, he may desire to repair his reputation by writing a better book, not by humiliating his critics; and if he attempts the latter rather than the former, he does so, not merely in order to enhance his "self-feeling," but because he is driven on by revenge.

In the feeling of gratification which results from successful resentment the pleasure of power also may form a very important element, but it is never the exclusive element. As the satisfaction of every desire is accompanied by pleasure, so the satisfaction of the desire involved in resentment gives a pleasure by itself. The angry or revengeful man who succeeds in what he aims at, delights in the pain he inflicts for the very reason that he desired to inflict it.

We have already noticed several facts which show that, in cases where the actual offender, at least for the moment, cannot be got at, or where some other feeling, especially fear, prohibits the sufferer from attacking him, resentment may be directed against some individual who is not even supposed to have inflicted the injury resented. These cases, however, which may be easily multiplied by every-day observation among ourselves, by no means vitiate the conclusion that resentment, as a means of defence or protection, is essentially directed against the being that caused the suffering which we resent. They only show the intimate connexion that exists between the experience of injury and the hostile reaction by which the injured individual gives vent to his passion, and which does not fail to appear even when it misses its aim.

That the fury of an injured animal turns against the real or assumed cause of its injury is a matter of notoriety, and everybody knows that the same is the case with the anger of a child. No doubt, as Prof. Sully observes, "hitting out right and left, throwing things down on the floor and breaking them, howling, wild agitated movements of the arms and whole body, these are the outward vents which the gust of childish fury is apt to take".[1] On the other hand, we know well enough that Mr. Darwin's little boy, who became a great adept at throwing books and sticks at any one who offended him,[2] was in this respect no exceptional child. That a similar discrimination characterises the resentment of a savage is a fact upon which it would be unnecessary to

[1] Sully, *Studies of Childhood*, p. 232 *sq.*

[2] Darwin, "A Biographical sketch of an Infant," in MIND, vol. ii., p. 288.

dwell unless there were some seeming anomalies that require an explanation.

It has been sufficiently proved that the blood-feud is an extremely wide-spread institution among peoples living on a low stage of social development. In this institution some sort of collective responsibility is always involved. If the offender is of another family than his victim, but of the same clan or tribe, some of his relatives may have to expiate his deed. If he belongs to another clan, the whole clan may be held responsible for it;[1] and if he is of another tribe, the vengeance may be wreaked upon his fellow-tribesmen indiscriminately. There is no difficulty, however, in explaining these facts. The following statement made by Mr. Romilly with reference to the Solomon Islanders has, undoubtedly, a much wider application: "In the cases which call for punishment, the difficulties in the way of capturing the actual culprits are greater than any one, who has not been engaged in this disagreeable work, can imagine".[2] Though it may happen occasionally that a manslayer is abandoned by his own people,[3] the general rule is, not only that all the members of a group are engaged, more or less effectually, in the act of revenge, but that they mutually protect each other against the avengers. A murder very often provokes a war,[4] in which family stands against family, clan against clan, or tribe against tribe. In such cases the whole group take upon themselves the deed of the perpetrator, and any of his fellows, because standing up for him, becomes a proper object of revenge. The guilt extends itself, as it were, in the eyes of the offended party. Moreover, because of the close relationship which exists between the members of the same group, the actual culprit will be mortified by any successful attack that the avengers make on his people, and, if he be dead, its

[1] Dr. Steinmetz says (*loc. cit.*, vol. i., p. 381) that he has found no instance of a blood-feud taking place between clans. My statement in the text is based on Bridge's account of the Fuegians in *A Voice for South America*, vol. xiii., p. 207; on Ridley's account of the Australian Kamilaroi in *Jour. Anthr. Inst.*, vol. ii., p. 268, and Godwin Austen's account of the Gāro Hill tribes of India, *ibid.*, vol. ii., p. 394.

[2] Romilly, *The Western Pacific and New Guinea*, p. 81. *Cf.* Friedrichs, "Mensch und Person," in *Das Ausland*, 1891, p. 299.

[3] *Cf.* Crantz, *The History of Greenland*, vol. i., p. 178.

[4] Dr. Post's statement (*Die Geschlechtsgenossenschaft der Urzeit*, p. 156) that the blood-revenge "charakterisirt sich . . . ganz und gar als ein Privatkrieg zwischen zwei Geschlechtsgenossenschaften," is not quite correct in this unqualified form, as may be seen, *e.g.*, from v. Martius's description of the blood-revenge of the Brazilian Indians, in his *Beiträge zur Ethnographie Amerika's*, vol. i., pp. 127-129.

painful and humiliating effects are still supposed to reach his spirit.

In spite of all this, however, the strong tendency to discrimination which characterises resentment, is not wholly lost even behind the veil of common responsibility. Thus Mr. Howitt has come to the conclusion that, among the Australian Kurnai, if homicide has been committed by an alien tribe, the feud "cannot be satisfied but by the death of the offender," although it is carried on, not against him alone, but against the whole group of which he is a member.[1] Concerning the West Australians, Sir George Grey observes: "The first great principle with regard to punishments is, that all the relations of a culprit, in the event of his not being found, are implicated in his guilt; if, therefore, the principal cannot be caught, his brother or father will answer nearly as well, and, failing these, any other male or female relative, who may fall into the hands of the avenging party".[2] In Wetter, according to Riedel, the malefactor is first sought after, and only if he cannot be found out is revenge taken on some other member of his *negari*.[3] Among the Fuegians, as we are told by M. Hyades, the most serious riots take place when a manslayer, whom one wishes to punish, takes refuge with his relations or friends.[4] Von Martius remarks of the Brazilian Indians in general, that, even when an intertribal war ensues from the committing of homicide, the nearest relations of the killed person endeavour, if possible, to destroy the culprit himself and his family.[5] Among the Guiana Indians, according to Mr. Brett, "if the supposed offender cannot be slain, some innocent member of his family—man, woman, or little child—must suffer instead".[6] With reference to the Creek Indians, Mr. Hawkins says that though, if a murderer flies and cannot be caught, they will take revenge upon some innocent individual belonging to the family of the murderer, they are, on the other hand, "generally earnest of themselves, in their endeavours to put the guilty to death".[7]

It is quite possible that much more to the same effect

[1] Fison and Howitt, *Kamilaroi and Kurnai*, p. 221.

[2] Grey, *Journals of Expeditions*, vol. ii., p. 239.

[3] Riedel, *De sluik en kroesharige rassen tusschen Selebes en Papua*, p. 434.

[4] Hyades and Deniker, *Mission scientifique du Cap Horn*, vol. vii., p. 375.

[5] Von Martius, *loc. cit.*, vol. i., p. 128.

[6] Brett, *The Indian Tribes of Guiana*, p. 357.

[7] Hawkins, in *Trans. American Ethn. Soc.*, vol. iii., p. 67. *Cf.* also Dall, *Alaska*, p. 416; Chalmers, *Pioneering in New Guinea*, p. 179.

might have been discovered, had only the observers of savage life paid more attention to this particular aspect of the matter. At all events, the most interesting point connected with the blood-feud is, not that the culprit himself often escapes so easily, but that, here again, the sufferings of innocent individuals are so utterly disregarded. It is in this point that a change of the utmost importance has taken place during the course of evolution. Can anything be more revolting to our feelings of justice, than the vengeance of the Californian Nishinam, who "consider that the keenest and most bitter revenge which a man can take is, not to slay the murderer himself, but his dearest friend"?[1] How contradictory to all our moral ideas, too, are the following facts. If, among the Marea, a commoner is killed by a nobleman, his death is not avenged directly on the slayer, but on some commoner who is subservient to him.[2] If, again, among the Quianganes of Luzon, a noble is killed by a plebeian, another nobleman, of the kin of the murderer, must be slain, while the murderer himself is ignored.[3] If, among the Igorrotes, a man kills a woman of another house, her nearest kinsman endeavours to kill a woman belonging to the household of the homicide, but to the guilty man himself he does nothing.[4] In all these cases the culprit is not lost sight of; vengeance is invariably wreaked upon somebody connected with him. But any consideration of the innocence of the victim is overshadowed by the blind subordination to that powerful rule which requires strict equivalence between injury and punishment—an eye for an eye and a tooth for a tooth,—and which, when strained to the utmost, cannot allow the life of a man to be sacrificed for that of a woman, or the life of a nobleman to be sacrificed for that of a commoner, or the life of a commoner to expiate the death of a noble.

A similar rule of equivalence, more or less rigidly enforced, not unusually regulates the practice of retaliation. Now it demands that only one life should be taken for one; now that a death should be avenged on a person of the same rank, sex, or age, as the deceased; now that a murderer should die in the same manner as his victim; now that various kinds of injuries should be retaliated by the infliction

[1] Powers, *Tribes of California*, p. 320.

[2] Munzinger, *Ostafrikanische Studien*, p. 243.

[3] Blumentritt, quoted by Spencer, *The Principles of Ethics*, vol. i., p. 370 *sq.*

[4] Jagor, *Travels in the Philippines*, p. 213.

of similar injuries on the offender. It may be well, for the right understanding of savage revenge, to give some further illustrations of this remarkable law. In Nukahiva, according to von Langsdorf, when a homicide has been committed, a family feud ensues, "but as soon as one is sacrificed, no matter whether man, woman, boy, or girl, the enmity ceases, and the most complete harmony is restored between the antagonists".[1] The Negrito and Igorrote tribes in the province of La Isabela, in the Philippine Islands, keep a regular *Dr.* and *Cr.* account of heads; and, strange to say, the same Igorrotes who requite the death of a kinsman by the death of some perfectly innocent individual, taken at random, are so very particular in this quasi-retaliation, that "for a dead man a man must be killed, for a woman a woman, for a child a child".[2] Again, in Abyssinia, if a man kill another, the murderer must be put to death by the nearest relatives of the deceased with precisely the same kind of weapon as that with which he killed his victim. Mr. Parkyns tells us to what a ridiculous extreme this principle is carried: A boy who had climbed a tree, happened to fall downright on the head of his little comrade standing below. The comrade died immediately, and the unlucky climber was in consequence sentenced to be killed in the same way as he had killed the other boy, that is, the dead boy's brother should climb the tree in his turn, and tumble down on the other's head till he killed him.[3] Other instances show that the law of equivalence does not refer merely to killing. Concerning the Indians of Guiana, Mr. Im Thurn states that, in theory, if not in practice, a complete system of tit-for-tat has saturated their minds, and that the smallest injury done by one Indian to another, even if unintentional, must be atoned by suffering a similar injury.[4]

We must not, however, believe that this strict equivalence is a characteristic of resentment as such;—in this point I agree with Dr. Steinmetz. There is undoubtedly a certain proportion between the pain-stimulus and the reaction; other things being equal, resentment increases in intensity along with the pain by which it is excited. The more a person feels offended the more intense (*ceteris paribus*) is his desire to retaliate the offence, and the more severe is the retaliation he seeks. Resentment, however, involves no accurate balanc-

[1] Von Langsdorf, *Voyages and Travels*, vol. i., p. 132.

[2] Foreman, *The Philippine Islands*, p. 213. Jagor, *loc. cit.*, p. 213.

[3] Parkyns, *Life in Abyssinia*, vol. ii., pp. 236-238.

[4] Im Thurn, *Among the Indians of Guiana*, p. 213 *sq.*

ing of suffering against suffering. Hence there may be a gross disproportion between the pain suffered and the counter-pain inflicted. Especially variable is the relation between the external action and the reaction, the exterior cause of resentment and the effect in which it issues. The same thing may call forth very different degrees of pain and resentment in different persons. The extremity to which anger may be driven in the bosom of a savage by an accident which appears to us as a trifle, is well instanced by the Patagonian cacique who, in a moment of passion, dashed his little three-year-old son with the utmost violence against the rocks, because he let fall a basket of eggs which the father had handed to him.[1] If, again, deliberate resentment is usually less excessive than sudden anger, it is so because there is time left not only for better estimating the extent of the hurt suffered, but also for other impulses to make themselves felt. Neither revenge nor sudden anger, however, stands in any naturally fixed relation to its cause. It may be sufficient to remember Hannibal who destroyed Himera and put to death 3000 male captives in revenge for his slain grandfather. Thus, while the direction of resentment against its cause belongs to its very nature, the exact demand of eye for eye and tooth for tooth does not. While some peoples are in the habit of taking only one life for one, others endeavour to destroy the whole family of the culprit.[2] While some only demand that the murderer shall die in the same manner as his victim, others seek to carry their revenge beyond death by mutilating the corpse of their slain enemy.[3] While some retaliate the various kinds of injuries by the infliction of similar injuries on the offender, others do not object to avenging even small injuries by death.[4] How, then, shall we explain the rule of equivalence, which regulates the revenge of some peoples, but which is not followed by others?

If this rule is not suggested by revenge itself, then of course it must be due to the influence of other factors which intermingle with this feeling and help, with it, to determine the action. One of these factors, I think, is self-regarding pride, which plays such an important part in the vengeance both of savage and civilised men, that it has, although mistakenly, been supposed to form the very essence of revenge. The

[1] King and Fitzoy, *Voyages of the* Adventure *and* Beagle, vol. ii., p. 130 *sq.*

[2] *E.g.*, the Brazilian Indians (von Martius, *loc. cit.*, vol. i., p. 128).

[3] *E.g.*, the Tasmanians (Calder, in *Jour. Anthr. Inst.*, vol. iii., p. 21).

[4] *E.g.*, the Timorese (Forbes, *A Naturalist's Wanderings in the Eastern Archipelago*, p. 473).

desire to pull down the humiliating arrogance of the aggressor naturally suggests the idea of paying him back in his own coin. Thus a kick is apt to call forth a counter-kick, a box on the ear a counter-box on the ear, a bad word another bad word in return, a destruction of property a counter-destruction of property. This similarity between action and reaction is undoubtedly due, at least to a great extent, to wounded pride, though it seems probable that the natural disposition to imitate, especially in cases of sudden anger, acts in the same direction. But besides this qualitative equivalence between injury and punishment, the *lex talionis* requires, in a rough way, quantitative equivalence. Now this demand cannot possibly have its origin in any of the factors just mentioned. The wounded "self-feeling" may easily claim that the punishment shall be at least equal in intensity to the insult, but at the same time it may lead to a retaliation far beyond that limit. Some other power, then, must have been at work when the law of like for like was established. Let us see what power it was.

It must be noticed that the strict rule of equivalence has the character of a custom, which, like all customs, is enforced by society. Revenge among savages is, indeed, not a matter of merely private concern; society is not a wholly indifferent bystander even when the offence committed is merely individual. Though the exaction of vengeance is generally described as a right belonging to the offended party or his group, there are facts, too numerous to quote, which show that, even among the lowest savages now existing, it is regarded as a social duty, and that an omission of it incurs general censure. Man is by nature both resentful and sympathetic. When he sees some of his comrades suffer injury or death at the hands of another individual, he feels pain and resentment himself, and, though not himself a direct object of the injury, he desires that the offender shall be punished. In this simple combination of resentment and sympathy we have a fact of extreme importance for the moulding of the moral consciousness,—infinitely more important than any calculation as regards social utility. If anybody makes the objection, that this explanation models the savage mind too much after our own, I may, choosing one out of innumerable similar instances, refer to Prof. Romanes's terrier which, "whenever or wherever he saw a man striking a dog, whether in the house or outside, near at hand or at a distance, . . . used to rush in to interfere, snarling and snapping in a most threatening way".[1]

[1] Romanes, *loc. cit.*, p. 440.

While public opinion thus demands that vengeance shall be exacted for injuries, it is also operative in another way. Whilst the resentment of the offended party may seem to outsiders to be, in some cases, too weak or too much checked by other impulses, it may, in other cases, seem to be unduly great. If the offender is one with whose feelings men naturally sympathise, this sympathy will keep the desire to see him punished, within certain limits, and if they sympathise equally with the suffering of the offender and with that of his victim, they will demand a punishment only equal to the offence. This demand—in combination with the rough idea natural to an uncultured mind that offence and punishment are to be measured by their external aspects—lies at the foundation of the strict rule of equivalence, which is thus an expression, not of unrestrained barbarism, but of advancement in humanity and civilisation. If this explanation be the correct one, the rule in question must have been originally restricted to offences committed by fellow-tribesmen, as public opinion could not otherwise have been an impartial judge. In speaking of the system of tit-for-tat prevalent among the Guiana Indians, Mr. Im Thurn expressly says: "Of course all this refers chiefly to the mutual relations of members of the same tribe".[1] When, on the other hand, we find, as we do, savages acting according to the same principle in their relations to other tribes, the reason for this may be sought partly in the strong hold which this principle has taken of their minds, and greatly in the dangers accompanying intertribal revenge, which make it desirable to restrict it within reasonable limits.

No facts then remain, so far as I know, which would contradict the view, hitherto so generally accepted, that resentment is essentially directed against its real or presumed cause. While the seeming exceptions to this rule have been shown to be due to the influence of other considerations to which resentment has been obliged to yield, innumerable instances might be put forth to prove the rule. I might in this connexion, for instance, refer to the practice of punishing "the offending member," which occurs among various peoples and is not unknown even among savages, but I shall restrict myself to saying a few words about another subject, which is of vast importance in the psychology of resentment.

Everybody knows that, among ourselves, at least, resentment is much more easily excited by intentional injury and by injury arising from negligence than by unintentional

[1] Im Thurn, *loc. cit.*, p. 214.

hurt. This is especially the case with deliberate resentment, which in fact seems impossible where no volitional cause of the hurt is assumed. Among savages again, as we find from trustworthy authorities, a distinction is often, but not always, and hardly ever with satisfactory preciseness, drawn between accident, *culpa* and *dolus;* and the history of penal law shows how slow and gradual the full recognition of this distinction has been among civilised peoples. All this may be easily explained from what has been said above about the natural direction of resentment, and the increasing regard paid to human suffering. The direction is against the presumed cause of pain, but for the discovery of the cause more intelligence may be requisite than is possessed by a savage. If my arm or my foot by mere accident gives a push to my neighbour, and he, after due deliberation, is perfectly convinced of my innocence, surely he cannot feel angry with me. Why not? Simply because he makes a distinction between a part of my body and myself as a volitional being, and finds that *I* am no proper object of resentment, as the cause of the hurt was merely my arm or my foot. Every man, however, is not able, or is not willing, to draw that distinction, and the result is what we call unjust resentment. This may be due either to low intelligence or to a craving, not sufficiently bridled by sympathy or moral considerations, for giving vent to the angry passion. Hence the difference between the resentment of an uncultured and that of a cultured mind, a difference which evidently does not touch the essence of that feeling itself.

Deliberation, however, may be carried still further. If a man has suffered wilful injury, he may come to think that it is unreasonable and cruel to desire to requite suffering by suffering, unless some good result—especially the removal of the bad will whence the original suffering sprang—seems likely to be thereby attained. He will probably find it difficult, perhaps impossible, altogether to submit to this voice of reason and sympathy, because, as we have already seen, there is a deep-rooted connexion between the desire to inflict counter-pain and the desire to remove the cause of the pain suffered. He will find it most difficult if he assumes the mischievous volition to be rooted in the man's whole character; he will find it easier if he can trace it back to some apparently accidental cause, such as insufficient knowledge or some physical disturbance. In fact, he has only to make a wider use of the lesson which his relation to the inanimate world has forced upon him. If he burns himself on a hot plate, he immediately tries to remove the cause of

his suffering, but he cannot reasonably desire to inflict counter-pain on a thing which can feel no pain. Of course, for a moment one may feel like Dr. Nansen, to whom, when he was crossing Greenland, it would have caused "quite real satisfaction," as he says, to destroy a sledge which was heavy to draw.[1] Such a desire, however, cannot last. Even the dog which hurts itself while playing with another dog, by running into a tree, changes its angry attitude immediately as it notices the real nature of the pain-giver.[2] In order fully to understand the difference between injury resulting from an inanimate thing (with which, to the enlightened mind, all sorts of accidental injuries are on a par) and injury inflicted by a volitional being, we must, however, also bear in mind that, in the former case, there is no exulting adversary who irritates us by his humiliating success.

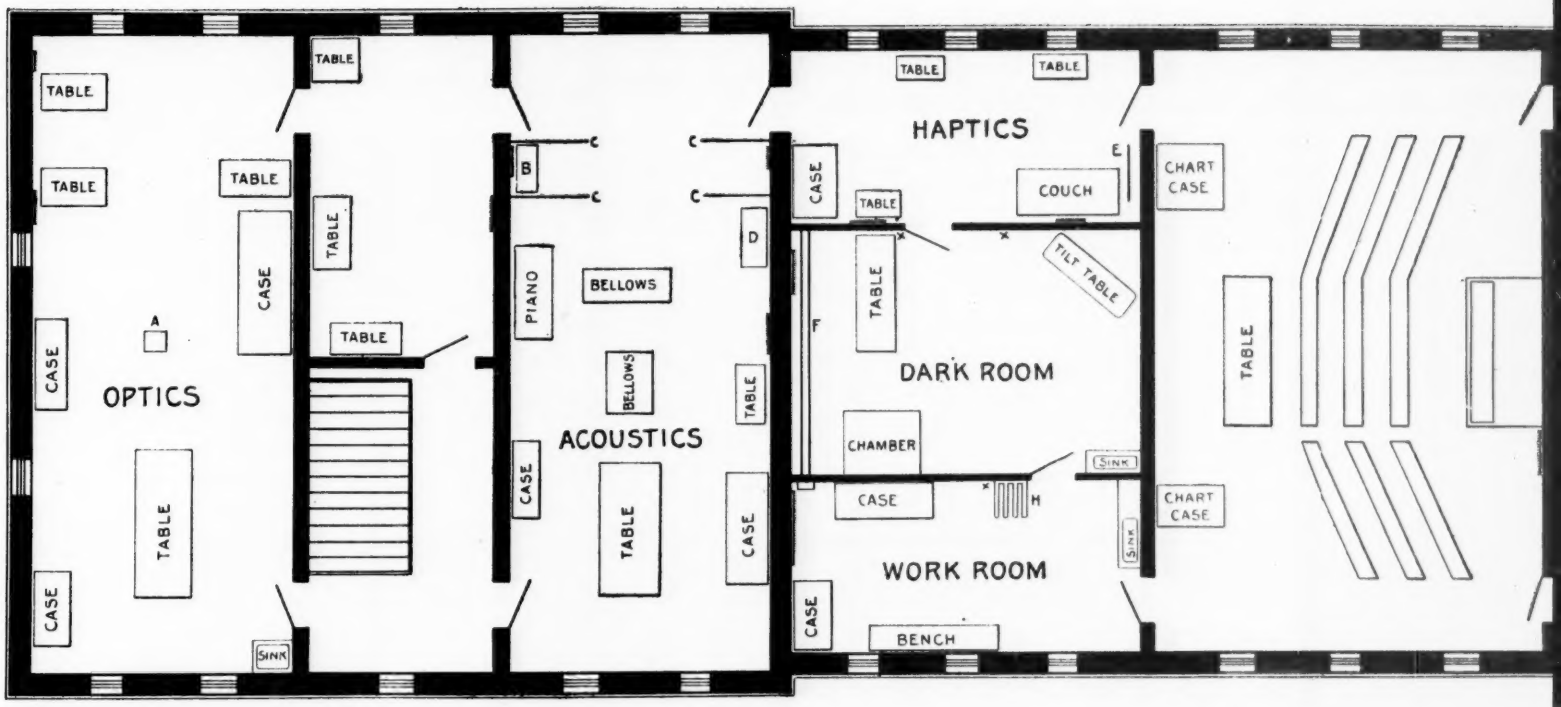
These last considerations have already brought us near the moral problem. It is in connexion with this that Dr. Steinmetz's theory seems to me perhaps most unsatisfactory. He himself finds it necessary to call in considerations of utility and social expediency in order to explain the direction of revenge and punishment, and from those considerations, I presume, we have then to explain the notions of moral guilt and responsibility. All this I consider to be fundamentally wrong. Responsibility, assuredly, has its root in a principle far deeper than the calm idea that a certain individual, *viz.*, the offender, should be sacrificed for the public weal; and the partition-wall which Dr. Rée and Dr. Steinmetz have erected between revenge and punishment is demolished by an overwhelming array of facts. Space does not permit me, however, to give reasons for my opinions on these important points, which, besides, do not fall within the scope of the present article.

In conclusion I wish to add a word about the method by the aid of which the conclusions here opposed have been arrived at. For my own part I consider it to be of vital importance for psychology to make much more use of the comparative method than it has hitherto done. At the same time it should use it with great care, and should especially try to avoid those mistakes in methodology which, in my opinion, encumber so many sociological works of recent date. Dr. Steinmetz has largely founded his psychological theory—which forms only a part of his, in many respects, important work—on cases of savage vengeance which he

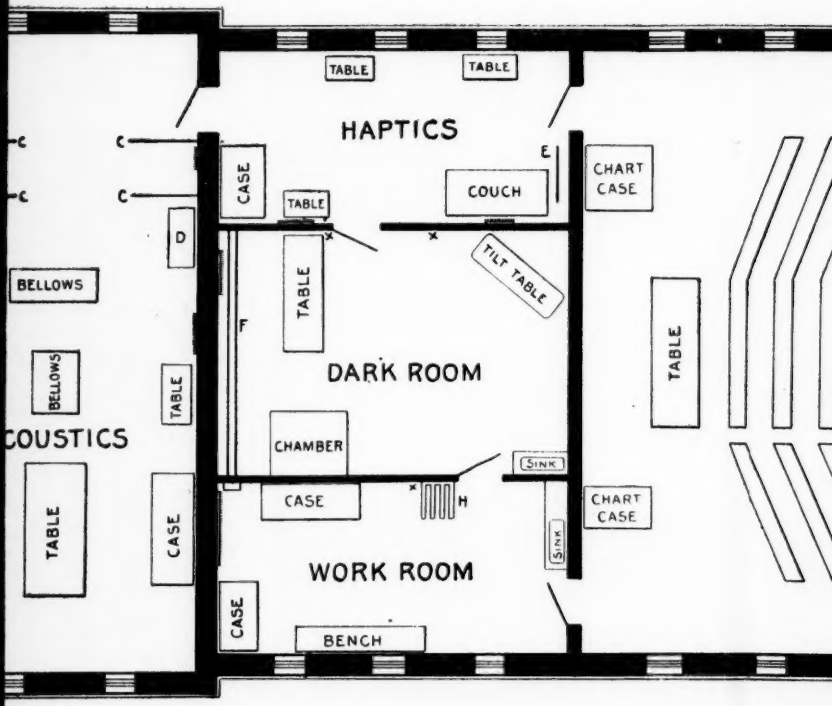
[1] Nansen, *Eskimo Life*, p. 213 *sq.*

[2] Hiram Stanley, *loc. cit.*, p. 154 *sq.*

quite arbitrarily and unmethodically, without ever consulting animal psychology, interprets as survivals from earlier stages through which the human race has passed. This, I maintain, is to introduce into psychology the most fatal error of modern sociology. The study of phenomena which may with any serious probability be regarded as survivals is indeed of extreme importance, and has already led to many brilliant discoveries; but, at the same time, I am of opinion that an uncritical survival-worship has given rise to a host of fantastic theories, which, in minds conversant with exact procedure, are unfortunately apt to weaken belief in the comparative method altogether. When I find among anthropologists that an ancient stage of universal polyandry is still affirmed with dogmatic certainty, on the ground that marriage with a deceased brother's widow is a custom of frequent occurrence; that *all* sacrifice is declared to have originated in a practice of eating with the god, preceded by a still earlier practice of eating the god himself; that universal totemism, with all sorts of presumed consequences, is becoming the religious dogma of a whole school, then I cannot wish comparative psychology to pass through a corresponding stage. Rather do I hope that the new science may be guided in its difficult course by the same judicious and truly scientific spirit that has made Prof. E. B. Tylor's great works the solid foundation-stones of historical anthropology.



PSYCHOLOGICAL LABORATORY



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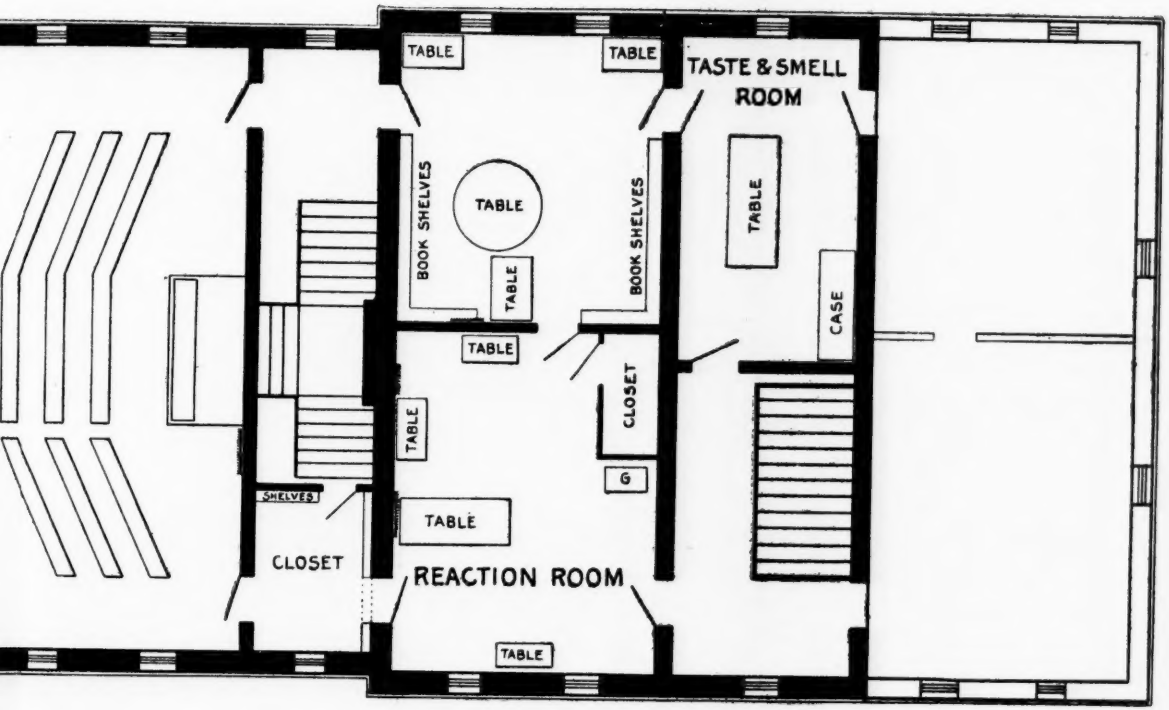

ABORATORY

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II.—A PSYCHOLOGICAL LABORATORY.

By E. Bradford Titchener.

It is now twenty years since Prof. Wundt instituted the first psychological laboratory in the University of Leipsic. A revolution, radical and far-reaching, was thus quietly inaugurated; the range and character of the psychological work of the last two decades differ in essential respects from those of the work that preceded it. There is probably no psychologist of note at the present day who does not confess to the influence exerted on his systematic thinking by the results of the experimental method, while there are not a few biologists, moralists and metaphysicians who acknowledge a similar indebtedness to laboratory psychology. Laboratories have been established in most of the principal universities of Germany and in all the principal universities of the United States, while there is, I think, no country now contributing to the general stock of scientific knowledge that does not possess at least one such foundation. It follows from all this that to carry on the psychological work of a modern university the psychological professor must have acquired a body of what one may call 'technical' knowledge, knowledge of applied mechanics and applied electricity, which a generation ago would have been as unnecessary for him as it still is for the professor of philosophy. For all laboratories are alike in their main features: there must be a power supply, special kinds of furniture and fixtures, and a collection of special appliances; and if physics, the mother

Explanaton of Figure.

The letters A to H of the Figure stand for the following instruments, etc.:—

A. Hering indirect-vision colour mixer.
B. Fall phonometer.
C. Curtains for screening phonometer and subject, to avoid echo.
D. Ellis-Helmholtz harmonical.
E. Münsterberg arm-movement apparatus.
F. Photometry bar.
G. Zimmermann chronograph.
H. Nichols rheostat.

The arrangement of tables, apparatus, etc., is largely arbitrary.

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of the sciences, does not preside at the installation, the provision of an equipment, however elaborate, will be in vain.

But although psychologists require from one another, and although universities require from their professors of psychology, some measure of familiarity with the principles of physics, and therefore with mathematics and applied physics, the kind and amount of this familiarity have never, so far as I know, been illustrated in the concrete by a description of the actual working of a psychological laboratory. Accounts of certain American laboratories have been published, one at least with a diagram of ground-plan and a statement of the dimensions of its rooms; there are papers dealing statistically with endowment, equipment, student attendance, etc.;[1]

[1] The following are, I believe, the most important references:—

(1) J. M. Baldwin, "The Psychological Laboratory in the University of Toronto". *Science*, O.S., vol. xix., 1892, p. 143.

(2) M. Baudouin, "La psychologie expérimentale en Amérique. Le laboratoire et les cours de Clark University à Worcester." *Archives de neurologie*, vol. xxviii., no. 89. "Les laboratoires et les cours à Yale, Harvard, Cornell, Pennsylvania, . . . etc." *Ibid.*, vol. xxviii., no. 93.

(3) W. Bechterew, "Le laboratoire psychophysique de l'université impériale de Kazan". *Cong. de Zool. de 1892*, vol. iii.

(4) E. Bérillon, "Notice sur l'institut psychophysiologique de Paris". Paris, 1897.

(5) M. W. Calkins, "Experimental Psychology at Wellesley College". *Am. J. of Psych.*, vol. v., p. 260.

(6) E. Casslant, "Le laboratoire de physiologie des sensations de la Sorbonne". Paris, 1897.

(7) E. B. Delabarre, "Les laboratoires de psychologie en Amérique". *L'année psychologique*, vol. i., p. 209.

(8) T. Flournoy, "Notice sur le laboratoire de psychologie de l'université de Genève". Geneva, 1896.

(9) V. Henri, "Les laboratoires de la psychologie expérimentale en Allemagne". *Rev. phil.*, vol. xxxvi., Dec., 1893.

(10) J. Jastrow, "The Section of Psychology". *World's Columbian Exposition Official Catalogue*, pt. xii., p. 50.

(11) W. O. Krohn, "Facilities in Experimental Psychology in the Colleges of the U. S.". *Report of the Commissioner of Education for 1890-1*, vol. ii., p. 1139. "Experimental Psychology at the Various German Universities." *Am. J. of Pysch.*, vol. iv., p. 585. "The Laboratory of the Psychological Institute at the University of Göttingen." *Ibid.*, vol. v., p. 282.

(12) "Psychological Laboratory of Harvard University." Cambridge, Mass., 1893.

(13) E. C. Sanford, "Some Practical Suggestions on the Equipment of a Psychological Laboratory". *Am. J. of Psych.*, vol. v., p. 429.

(14) G. M. Stratton, "The new Psychological Laboratory at Leipzig". *Science*, N.S., vol. iv., 1896, p. 867.

(15) H. de Varigny, "Le laboratoire de Madison, Wis." *Rev. scient.*, 1894, p. 624.

Of these papers (all of which, except nos. 3, 4 and 6, are known to me)

and the output of the Leipsic laboratory has twice been summarised in the pages of MIND.[1] But I know of no article which describes the 'running,' the daily working, of a psychological laboratory. This is the topic with which I propose here to deal, making the arrangements of my own laboratory the peg upon which to hang my account. I hope that the results of my experience may be of some use to my colleagues, and especially to those whose laboratories are still in the planning; and I hope, further, that what I have to say may call out comment and criticism, and so lead to a general formulation of the necessities of an adequately equipped laboratory.

First of all, however, and by way of preface to the whole discussion, I would remind the reader of the dual character of the American psychological laboratory. The German laboratories are essentially research laboratories; drill-courses are practically unknown. 'Einführungscursus' are given at some universities; but the 'Einführungscursus' at Leipsic, *e.g.*, consists simply of a series of lectures upon the psychophysical measurement-methods, photometric methods, and reaction methods, with occasional exercises and demonstrations. The student gets his training by serving as 'Versuchsobject' for his seniors, and the training varies as the investigations in progress vary. If he desires to repeat the classical experiments in any particular field, he must do so on his own account. The American laboratory, on the other hand, has to serve the purpose of instruction as well as that of research. Any scheme of arrangement and equipment must, therefore, keep the drill-course steadily in view.

This difference in educational purpose and conditions can be brought out most clearly by a description of the psychological curriculum at a typical American university. At Cornell, *e.g.*,—I select the course with which I am most familiar,—the full curriculum extends over a period of six years. In his second undergraduate year the student can take a course in General Psychology (lectures and demonstrations); in the following year, a drill-course in Experi-

nos. 5 and 13 come nearest the intention of the present article. I omit a few papers of merely local interest or popular character. Information as to laboratory furnishings can be gleaned from many other sources; thus there are accounts of a 'silent room' and of a photographic room in the *Yale Studies*, of a dark room in the *Phil. Stud.*, the *Arch. f. Ophth.*, etc.; Prof. Sanford's *Course* has a chapter entitled "Suggestions on Apparatus"; the fourth ed. of Wundt's *Phys. Psych.* devotes much space to the description of instruments; and so on.

[1] By Prof. Cattell, in O.S., vol. xiii., p. 37; by myself, in N.S., vol. i., p. 206.

mental Psychology (laboratory work and occasional lectures); in the fourth year, a course in Systematic Psychology (lectures, demonstrations, exercises). No lecture courses need be taken in the graduate years, though some special course or seminary is usually attended. The work done after the obtaining of the B.A. degree is, therefore, almost exclusively, research work. At the end of one year of graduate study, the M.A. degree may be taken; two years later, the degree of Ph. D. The gap between the undergraduate and graduate curricula is bridged by the requirement of a thesis for the first degree. Here, then, are three years of instruction and three years of investigation. And—what is the important point in the present connexion—all students, from those of the third undergraduate year inclusive, are handled not in a general lecture-room, but in the laboratory.

The consequences of this twofold demand upon the laboratory will appear in detail as we proceed; it is sufficient here to have called attention to the fact. I pass on, therefore, to a description of the Cornell laboratory itself.

1. *General Plan of the Laboratory.*—I do not know that any psychologist has hitherto succeeded in persuading his university to provide him with a special building, constructed after his own designs. The directors of the new laboratories at the University of Leipsic and at Columbia University were, I believe, allowed complete freedom as regards arrangement and equipment; but, none the less, these laboratories form parts of large buildings, so that their plan is necessarily determined, to some extent, by the plan of the whole. At Yale a private house has been converted to laboratory uses; at Toronto the laboratory occupies two widely separated sets of rooms in the main university building; everywhere, indeed, psychology has come late to the feast, and been obliged to content itself with what it can get.

The Cornell laboratory was opened in 1891. From that date until 1895 it consisted of a continuous suite of six not very large rooms. Extension then became urgently necessary, and it migrated to its present quarters,—the upper floor of one of the university lecture halls. It now consists, as the Figure shows, of ten rooms, to which, I hope, the two remaining rooms of the floor will soon be added. These two rooms are shown in the Figure, as the laboratory has been planned—partitions erected, doors cut, etc.—upon the twelve-room basis. It is accessible from three corridors; those to the north and south are used principally by graduate students, while the central stairway, opening upon the lecture-room, is the way of public entrance.

Room 1, the 'optics room,' is a large room, lighted from three sides, with walls and ceiling painted a dull cream. Room 2, intended for the private room of the laboratory assistants, now serves the purposes to which room 12 will ultimately be put. Room 3 is the 'acoustics,' room 4 the 'haptics room'. Room 5 is a dark room employed for drill-work, demonstration and photography. Room 6 is the 'work,' and room 7 the 'lecture-room'. Room 8 is the director's private room; room 9 the 'reaction,' and room 10 the 'taste and smell room'. Room 11, which faces north, will be fitted up as a research dark room; room 12 will be furnished with the instruments used in the investigation of the physiological processes underlying affective consciousness, —pulse, respiration, volume and muscular tone. Between rooms 7 and 9 is a large store-closet, spacious enough to allow of the building up of apparatus previously to its actual use in the appropriate room. It will be noticed that the work-room is entirely cut off from the acoustics room, and opens only into rooms 5 and 7. The reaction room contains the time-measuring instruments and their controls; it is paired with the optics, acoustics, haptics or lecture-room for the performance of reaction-time experiments.

It may be said at once that the above arrangement has proved quite satisfactory; I do not know how the space could be more usefully apportioned. The haptics room is too small; but it could not have been made larger without very considerable outlay, and crowding is avoided by pairing it with the work-room, especially for temperature work. We are suffering just now for lack of a few more small rooms, but the pressure will be relieved when the full plan of the laboratory is realised.[1]

2. *Furniture.*—Rooms 1, 3, 4, 9 and 10 each contain one or more large wall-cases, for the storage of the optical, acoustical, haptical, reaction and taste-and-smell apparatus, respectively. It need hardly be said that the instruments thus grouped under various sense-names are by no means exclusively sensation instruments. The optical apparatus, *e.g.*, includes instruments for the investigation of visual sensation, visual space perception, visual memory, visual recognition, visual attention, imagination, etc., and for the giving of visual stimuli, simple and compound, in reaction work; the acoustical

[1] The 'when' here and elsewhere in the article should perhaps be written 'if'. M. Flournoy, whose original account of the Geneva laboratory proved to be too sanguine, remarks sorrowfully that "il ne faut jamais vendre la peau de l'ours avant de l'avoir tué". The fact that I annex rooms 11 and 12 solely for the reader's benefit may, I hope, suffice to propitiate the Fates.

apparatus includes the instruments necessary for the investigation of tonal fusion, of clang analysis, of auditory rhythm, of auditory memory, attention and recognition, of the localisation of sounds, etc., and for the giving of auditory stimuli in reaction work. The distribution of the various appliances to their appropriate rooms has some convenience, as saving the labour of transportation from one part of the laboratory to another. But I am quite clear upon the point that the ideal laboratory must have a central 'museum room,' for the storage of all pieces not actually in use.

Small, solid, low tables are necessary for haptical work, and convenient for olfactometry. They are found in rooms 4 and 10. Where mercury is used (I shall return to this question presently) the tables must be edged with a raised bead, and a shallow well sunk in one or more corners, to prevent spilling. Tables of this sort have been placed in rooms 1, 3 and 9. It is advisable to have a lounge and a couple of swing-back, revolving chairs in the laboratory; very much depends in research work, and particularly in haptics, upon the bodily comfort of the subject.

The work-room has two wall-cases, the one for batteries, the other for general laboratory supplies, and a carpenter's bench with the usual appliances. The south end of the lecture-room is taken up with two cases for wall-diagrams and a long draughting table. The nine desks or benches shown in the Figure—each of which accommodates four students, separately seated—are low and extremely solid, so that class experiments can, if need arise, be performed upon them.

Besides tables and chairs and cases, which are 'furniture' in the strict sense of the word, the laboratory possesses certain pieces which lie upon the border-line between furniture and apparatus, and which may therefore be mentioned in this place. In the optics room, *e.g.*, we have a wall-campimeter, 4.25 x 1.80 m., made by slinging a wooden frame upon ordinary copper window-springs between uprights that reach from floor to ceiling. The frame is easily reversible, and is covered on the one side with black, on the other with white cloth, the meridians being marked out with grey and black threads respectively. This whole apparatus cost only £2, and has proved eminently serviceable.[1] In the acoustics room, again, we have the Appunn and Koenig

[1] Throughout this article I take £1 as the equivalent of $5.00.—It would, of course, be easy to replace the thread-meridians by a revolving arm of the same brightness as the background. But the apparatus would then be a good deal more expensive, and I do not think that any real advantage would be gained.

bellows tables, a strong, portable foot-bellows, and an arrangement whereby a section of the room may be curtained off for phonometrical work. The fittings of the dark room are almost wholly of this mid-way character. Along the south wall runs a photometric bar, ending to the west in a dark box within the dark room, and to the east in a similar box opening into the work-room. Parallel with this bar extends the association apparatus, consisting of a long black table that carries the lantern and ground-glass screen (west), and a small, black, curtained chamber, within which the subject sits during experimentation (east). The centre of the room is free. To the north of the two doors we have the combined tilt-table and rotation-chair (west), and the photographic sink and ruby-window (east). The future research dark room, number 11, will have a Hering window, and heliostat arrangements for solar work.

3. *Gas, Water and Electric Service and Fixtures.*—Water is laid on in rooms 1, 5 and 6; gas in rooms 1, 3, 5, 6 and 7. Both services can easily be extended, if need arises. We have made no trial of water as a source of power.

The laboratory possesses three sets of electric fixtures. (*a*) It is furnished throughout with incandescent lamps for illuminating purposes. These lamps are, at present, run upon a night circuit only. Hence we have not employed the alternating current as a source of power. (*b*) Each of the rooms 1 to 9 is connected with every other room by a thirty-fold system of overhead wires. These are of varying sizes, the smallest capable of carrying a six-ampère current without undue heating, and mounted and insulated for a pressure of twenty-five volts. The wires come down to switch-boards at the places indicated in the Figure by a dotted line. The two half-systems—the groups of thirty wires running to the north and south ends of the laboratory—are brought together at a double (sixty-wire) switch-board in room 6. We are thus able to close twenty simple circuits at the same time.

There are three things for which a laboratory is disquieted: primary batteries, smoked paper and mercury.[1] The ideal

[1] At the Christmas, 1897, meeting of the American Psychological Association, Prof. Cattell remarked: "We have tried to banish mercury and smoked paper from our [the new Columbia] laboratory; and we think we have succeeded". I have not yet succeeded in finding the perfect substitute for smoked paper in chronographic work; but there are so many writing devices upon the market that a systematic search will probably reveal (or at least suggest) something that can advantageously replace it. Where a current can be passed through the writing point to some part of the writing surface, tinsel paper ('silver' paper) may be employed.

laboratory will be able to dispense with them; I am sorry to say that we have still some need of all three. Primary batteries we employ only for the ringing of signal bells and for our telephone circuit; the Hipp chronoscope can be worked satisfactorily by a storage battery. Of course, a primary cell may be useful for some occasional purpose; but as a general rule it is more troublesome than serviceable. Mercury we employ on our small switch-boards. These are constructed after the Leipsic model. The wires descend to a wooden shelf in which is bored a series of wells. Over against these, in the length of the shelf, a counter-series is sunk, from which wires run to the apparatus. The wells are filled with mercury, and connected according to requirement by wire bridges. All shelves are edged with a raised bead, so that there is no lateral escape of mercury drops; and each is furnished with a close-fitting lid, which is kept always in place except for the brief intervals during which connexions are made or shifted. Our central switch-board has only dry contacts; and I should now prefer to have all wet contacts, whether on switch-boards, keys, or instruments, replaced by hard metal.

(*c*) The direct current supplied by the university power plant enters the laboratory at the haptics room, and is available for use at two points in the dark room and in the work-room (see **x** in the Figure). We draw upon it to charge storage batteries, to run the arc-light of our lantern, and in general to actuate instruments (colour wheels, pendulums, kymographs) throughout the laboratory. The necessary reduction of current and pressure can be effected either by the arrangement of lamp resistances to which Dr. Scripture has given the name of 'lamp batteries,' or by aid of a Nichols tinned iron rheostat.[1] I have no hesitation in recommending the latter in preference to the lamp batteries. The initial cost of the rheostat is very small; it is not liable to be broken, as lamps are; and the required amount of pressure and current can be obtained very easily and very accurately. As many pairs of wires as are needed can be taken from the rheostat to the central switch-board, and instruments supplied simultaneously in the different rooms of the laboratory. There is, naturally, some waste of power in the use of a heavy current for laboratory ends; but this is more than counterbalanced by the saving of time, trouble and expense that follows from the elimination of the primary battery.

[1] E. W. Scripture, *The New Psychology*, 1897, pp. 483, 484; E. L. Nichols, *The Outlines of Physics*, 1897, pp. 442, 443.

So far, we have not found it necessary to have the current 'on tap' at more than the three places indicated. The main lead can be extended to other rooms at any time. Next year I intend to cut in the north wall of the dark room a square opening for the reception of a sheet of ground glass. Behind this a projection lantern will be placed. It will thus be possible, by arranging the audience with their faces to the south, to show slide diagrams in the lecture-room without darkening the room itself.

4. *Books.*—Books are necessary adjuncts to laboratory work; but it is difficult to give anything like a precise estimate of the library required. We are fortunate, at Cornell, in this regard: Morrill Hall, in which the laboratory is situated, lies within a stone's throw of the University Library. The psychological department has a yearly grant of £15 for the purchase of books for the library, and another of some £2 or £3 for the placing of books in the Philosophical Seminary Room. These grants are distinct from the library periodical fund. As for the laboratory: room 8 contains my private library, which has a fair working collection of psychological books and pamphlets, and files of the more important neurological and psychological periodicals. It is, of course, at the disposal of students who are working in the laboratory. I should say, giving a very rough estimate, that an initial sum of £20 and a yearly expenditure of £10 would provide a moderate technical library for laboratory purposes, inclusive of periodicals.

There is, however, one class of books which the head of the laboratory must collect for himself,—catalogues. Catalogues may usually be had for the asking; and all sorts and kinds, chemical, electrical, physiological, physical, bacteriological, photographic, zoological, are apt to 'come in useful' at one time or another. An apparatus should never be ordered after consultation of a single catalogue; it may quite well be that the tenth or eleventh that one consults will advertise a more serviceable form of the same device. And no apparatus that is at all costly should be ordered without previous consultation with the director of a large laboratory. Almost every firm has its weaknesses and its excellences, which can be discovered only by experience. The older laboratories have bought this experience, and it is needless for their successors to pay the same price. I may add a word of advice to inquirers: that they make their questions as *definite* as possible. Scientific men are, as a rule, only too glad to help one another; but to require your correspondent to formulate your problem,

as well as appreciate your instrument, is to demand too much.[1]

5. *Instruments.*—It is not my intention here to give a catalogue of our own instruments. A year or two hence, when the laboratory is completed, I hope to publish a pamphlet containing a full list of the pieces that we possess, with makers and prices, and stating briefly the results of our experience with them. At present we are concerned, however, not with a particular laboratory, but with the working of the laboratory in general, and have to consider psychological appliances from a general standpoint.

Looked at in this way, the instruments fall into four, unequal but distinct, groups. We have (*a*) the apparatus needed for research in experimental psychology; (*b*) that required for drill-work in experimental psychology; (*c*) that required for class experiments in experimental psychology; and (*d*) that employed in the study of individual psychology.

(*a*) A large proportion of the pieces listed as serviceable for research in psychology and the physiology of the senses are pieces that have been employed in a special investigation for a special end. I suppose that all original work, in whatever science, is likely to require newly constructed apparatus. But the older sciences are at this advantage, that they have a large store of designs to draw upon; so that the new appliance may take shape, at least in large measure, from the recombination and readjustment of older devices. Experimental psychology is still so young that each new problem must be faced independently, and instruments contrived to meet its peculiar exigencies. The consequence is that a laboratory which is to keep abreast of scientific progress must have a skilled mechanic attached to it. It is of little use to have ideas, if you have no means of realising them in brass and steel. This is the weak point of the Cornell laboratory: we have no mechanic of our own, and no skilled workman to whom we can have recourse in the city. A laboratory so situated is very severely handicapped. For successful work we should have a workshop, fitted up at a cost of £150 or £200, and a mechanic working at least half-time for the laboratory.

But it is not only necessary to devise apparatus for the solving of new problems; even the standard pieces usually require a thorough overhauling on their arrival in the labora-

[1] A useful preliminary list of firms will be found in the pamphlet issued by the Harvard Psychological Laboratory, to which I have referred in a previous note.

tory. Mr. J. D. Brown points out in his catalogue[1] that the old-established Hipp chronoscope generally demands a re-adjustment after it has left the factory. And if this is true of the dry tree, what may be expected of the green? A clever carpenter will go a long way in a laboratory, as I can gratefully affirm from personal experience; but what is needed is a skilled metal-worker who is something of an electrician and does not disdain to work in wood when wood-work is desirable.

(*b*) I mean by 'drill' instruments the appliances needed for such a course as is offered at Cornell in the third undergraduate year. The first term in this course is devoted to qualitative work, the repetition of classical experiments in optics, acoustics and haptics. The work of the second term is quantitative, work with the psychophysical measurement-methods. That of the third term is both qualitative and quantitative; it includes experiments upon action, attention and the affective processes.

The apparatus used in such a course is almost wholly American, just as the research apparatus is preponderantly German in origin. There are two or three American firms from which one can procure the instruments needed for Prof. Sanford's *Laboratory Course in Experimental Psychology*. Individual pieces are, however, by no means always satisfactory; and no firm, as far as I am aware, has received Prof. Sanford's authorisation. For optical work we have the very useful collection of material put together recently by Prof. Münsterberg.[2] For haptical, there are a number of pieces upon the market bearing Dr. Scripture's name.[3] Some of these are quite good; others (I speak from three years' experience) are practically valueless. I have no doubt that, with revision, this set could be made both more efficient and less expensive than it is. For acoustical work we have, as yet, hardly any drill material. I am now trying to make up a fairly cheap and reliable acoustical outfit for class purposes, though I shall be very glad if some other psychologist anticipates me.

The method work presents little difficulty. There are plenty of pieces lying on the border-line between this and the previous group that can be put into the hands of careful students and give good results. I need only mention colour wheels,

[1] "Catalogue of Phil. Instr. devised by J. McK. Cattell," Camden, N. J. *Cf.* Prof. Cattell, in *Mem. Nat. Acad. Sci.*, vol. vii., no. ii., 1896.

[2] "Pseudoptics," Milton Bradley Co., Springfield, Mass., £1.

[3] *Cf.* Catalogue of E. G. Willyoung & Co., Philadelphia, Pa.

eye-measurement screens, weights, movement apparatus, forks and temperature tubes.

The reaction work has hitherto offered considerable difficulties. I believe (though this is not the place to enter into technical reasons) that the Hipp chronoscope arrangement is the most satisfactory of any yet proposed. And it has the advantage, for drill work, that the student need not know anything about the special mechanism of the clock; he may receive a useful training, both as subject and as experimenter, although he has but the vaguest knowledge of the technicalities of the apparatus. On the other hand, the chronoscope is expensive; I do not suppose that any laboratory possesses more than two clocks. Hence, while four students may be engaged at any one time upon reaction experiments, the rest of the class must occupy themselves with other matters until their turn comes round. The difficulties seem happily surmounted by the introduction of Prof. Sanford's vernier chronoscope. This instrument costs only £1, and is well adapted for drill work.[1]

(*c*) Class experiments are very different from the drill experiments just described. In drill work the students are paired off, two and two, precisely as they would be in research work; the only distinction is that the experiments are repetitions of experiments already performed, and that they are repeated with somewhat rough instruments,—instruments that are cheap enough to be bought by the half-dozen. The class experiment is an experiment in which ten or a dozen students take part at the same time. It is necessitated by an overcrowded laboratory and a small teaching staff.

We have succeeded in keeping the numbers of our third year's class down to twenty; the course is a hard one, not to be taken except by those seriously interested in psychology. The reason is—and this may be taken as an axiom of psychological pedagogy—that the instructor cannot handle more than ten, and cannot well handle more than eight students during a two-hour laboratory period. To let more into the laboratory without a corresponding increase of the teaching force would be unfair both to teacher and to learner. But there are many teachers who shrink from any system of rigorous exclusion. To them two alternatives are open. The one is to repeat their work, giving the course six days in the week instead of three; the other is to have recourse to class experiments. It seems to me that the first plan is

[1] *Am. Journ. of Psych.*, vol. ix., p. 191.

objectionable from the point of view of the director of the laboratory: it curtails unduly the opportunities for research work, and brings teaching too much to the front; while the second is objectionable from the point of view of the student: he receives no individual instruction, and has to perform his experiments in a disturbing environment, and without any of that freedom of speech and movement which is one of the great charms of true laboratory work. Hence I can say nothing, from personal experience, of class experiments. One such arrangement would be a 'chain reaction' taken by the help of Wundt's demonstration chronoscope; another would be a test of the range of attention by means of the large fall chronometer. At the Christmas meeting of the American Psychological Association a paper on this subject, written by Dr. Kirschmann of the University of Toronto, was read by title. When it is published we shall know more about the possibilities of the class experiment and of the appliances that it requires. In the meantime there can be no doubt that many laboratories already possess appliances of the sort, and that they will play an increasing part in the work of those that are poorly endowed or much cramped for space.

(*d*) It is through individual psychology that experimental psychology joins hands with anthropometry. 'Mental tests' are beginning to loom large in psychological literature, and every test requires its own apparatus. The instruments must be strong, cheap, and of simple construction. A good many of those devised by Mr. Galton would answer the purposes of individual psychology were they less expensive. The best pieces to be had at present, so far as I am acquainted with them, are of Prof. Jastrow's contriving. A full set would include the ordinary sense tests (tests of keenness of vision and of audition, of colour blindness, of æsthesiometric discrimination, of power of smell, etc.), instruments for the taking of the simple and association reaction-times, tests of muscular strength, steadiness, fatigue, etc., as well as tests of the 'higher' mental processes: quickness of apprehension, ingenuity, accuracy, fidelity of memory, power of co-ordination, keenness of observation, type of memory and imagination, control of attention, temperament, mental disposition and furniture, etc. The materials for these last cover a wide range, extending from ink-blots upon paper squares to the crowning triumphs of 'parlour magic'. Whether mental tests, as at present performed, will yield a real insight into the tangle of the individual consciousness is a question that can hardly be answered for some time to

come. The significant thing is that many psychologists are seriously interested in them, and that this interest has brought to birth a fourth category of psychological apparatus.

The ideal laboratory will possess, in its museum room, a collection of instruments of all these four classes. The working laboratory does not need to possess them all. As I have indicated, we have ourselves no pieces of class *c*, which is a substitute for class *b*. We are now getting together some amount of the material that comes under class *d*, but rather with a view to testing its general psychological value than of carrying out any investigation into individual psychology as such. Experimental psychology must legislate for individual psychology, but the aims of the two are different.

6. *Endowment.*—A laboratory can be equipped, one may say, at any cost, from a final £10 upwards. I have several times been called upon to lay out sums of £10, £20 or £30 for the less fortunate of my colleagues; and something of value can be obtained even for the smallest of these amounts. Hence my advice to the as yet unendowed would be that they take what they can get, but get all that they can. If I were compelled to name a definite limiting sum, I should, I think, make it £30. For £10 you can get one or two good pieces: the Ellis-Helmholtz harmonical, or the Marbe colour mixer, or a Zwaardemaker double olfactometer with solutions, or a triad of colour wheels and a reliable æsthesiometer, or a Stumpf interval apparatus, or a set of Appunn forks; and the remaining £20 will go some distance towards a series of instruments for drill work, more especially if you have a carpenter at hand and make your memory-drops and fall chronometers and so forth at home. Still, a psychologist thus endowed could scarcely be called happy during his lifetime.[1]

The Cornell figures may be used to give the reader an idea of endowment upon the university scale. In 1891 the Professor of Psychology was granted a sum of £400; this was spent upon standard pieces and sets, not upon furniture and

[1] Professor Sanford has made out a liminal list of apparatus—the amount that is just noticeably better than nothing—as follows: "Weights for pressure and lifting, including those of equal weight but unequal size; a sonometer; ten or a dozen ordinary tuning-forks of a′ and c″ pitches for special tuning; a resonance bottle; a pair of bottle whistles; a yard of small rubber tubing; Bradley's Pseudoptics; some small pieces of coloured gelatine; a 60° prism; a dark box; and a couple of yards of metric cross-section paper". *Course*, p. 419. The approximate cost of such a set would be £5.

fixtures. The laboratory is permanently endowed with an income of £120, the annual payments of which began with the academic year 1891-2. This £120 is supposed to cover fixtures, running expenses, etc., as well as apparatus. Two grants have, however, been made in addition to it: one of £53 for installation of the equipment in Morrill Hall in 1895, and another of £21 for the taste and smell room in 1897. At the end of the present academic year, 1897-8, the expenditure upon the laboratory will therefore amount to £1194, irrespective of minor gifts of instruments from students and from the director.[1]

Arguing from our own experience, then, I offer the following sums as roughly representing the upper and lower limits of requirement for a university laboratory, on the assumption that rooms, light, heat and power are free:—

Initial expenditure; workshop -	£175,	£100.
Initial expenditure; apparatus -	£600,	£400.
Annual income; apparatus - -	£150,	£100.
Annual income; books - - -	£20,	£10.
Annual income; mechanic's salary,	?	

The latter item will depend upon the facilities offered the mechanic for putting the pieces invented in the laboratory on the general psychological market, and the vogue that these pieces obtain, as well as upon the number of hours a day that he devotes to the laboratory.

Where does all this money go to? It goes, first of all, for research apparatus. Every autumn we purchase, at Cornell, some few good instruments, choosing them, so far as possible, with a view to our own future investigations, but attempting, at the same time, to give equal attention to all the different fields of work. This year, *e.g.*, we bought the Koenig bellows table and differential sonometer, the new Griesbach æsthesiometer, a Dove siren, and the Hering indirect-vision colour mixer and colour-blindness instrument. The total cost of these, delivered in the laboratory, was rather more than £50: no small slice from the year's income. Indeed, all research apparatus is costly. Our chronograph, with accessory instruments, cost us over £60, transportation not included. The acoustics room contains no less than eighty-three forks, of varying sizes, all of them used in the course of a year's work for one purpose or another; and forks are not cheap. The large Helmholtz colour-mixing apparatus, which, I believe,

[1] To the psychologist, £1000 seems a large sum. What would it seem to the physicist or the physiologist? The comparison shows very prettily the working of the law of the *fortune morale*.

the Leipsic Institute alone of psychological laboratories possesses, is listed at Mk. 3000: and so on.

Secondly, the money is spent upon drill instruments. The expense here is not large, after the first initial outlay, although repairs amount to something every year. The only considerable items in our 1897-8 sheet are two, of £4 10s. for six Sanford chronoscopes, and of £3 for two Roux regulators (temperature work). I do not expect to spend upon drill appliances more than £10 a year at the outside, for some time to come.

There remains of our £120 a sum of, say, £60. This is accounted for under the very elastic heading of 'running expenses'. The items vary almost indefinitely from year to year. A laboratory is constantly needing glassware, photographic supplies, rubber tubing, the commoner carpenter's tools and materials, the minor electrical fittings (binding posts, switches, fuse plugs), drawing materials (pens, paper scales, drawing inks, crayons), black and white cardboard, clock oil and chamois leather, clamps and supports—a thousand and one things, from beeswax and burners and burettes to T-ways and thermometers and twine. These 'mount up' to a higher total than might be supposed. Next in importance, perhaps, stand the payments for carpenter's labour. There is hardly a week in the year that we have not something for the carpenter to do; he must alter fixtures, make frames and screens and shutters, overhaul the woodwork of foreign instruments (we find that all imported wood, however paraffin-soaked, warps and splits in time), cut windows in partitions, construct models and demonstration pieces to design,[1] stop leaks in the dark room, make up resistances, turn weight-holders, mend bellows, etc. I said above that a clever carpenter is capable of rendering essential aid in the laboratory. Besides the wall-campimeter, of which I have already spoken, we have a reading apparatus, an artificial waterfall, a Lambert colour mixer, a Listing's Law screen, a telestereoscope and several other useful pieces, all carpenter made. Thirdly, there are the expenses incident to investigation; the renewal of damaged parts, the supply of kymograph paper, of gelatine sheets, of taste and smell solutions, of the materials required for work in individual psychology,—alphabets and numerals, ruled cards and printed sheets,

[1] I made no fifth class of apparatus, for 'demonstration' instruments, because the pieces used for lecture demonstrations will always, I think, fall within some one of my four classes, or be simply models; models of brain or sense organs or sensori-motor connexions, or models illustrative of the course of association, of feeling, etc.

photographs and plaster casts. And lastly come the purely sporadic expenditures, for occasional anatomical specimens or microscope slides, for preliminary models of instruments to be made in metal, for drawings of instruments or wall-charts, and so forth. Anything that remains at the end of the year goes towards the purchase of research instruments.

The reader must not imagine that the line between 'instruments' and 'running expenses' can be very sharply drawn; still less that a hard and fast division can be made between the various kinds of 'running expenses'. I have had recourse to a rough classification for purposes of survey, but that is all. In reality, a good part of our 'running expenditure' inures to the benefit of the laboratory; many of the items on the list are not perishable. The laboratory has just now to pay the penalty of its youth. Later on, when the sundries are no longer all to get, the running expenses will diminish, even if the general demands upon our resources should considerably increase. It is the first few years that cost.

7. *Conduct of the Laboratory.*—The laboratory is opened at 8 A.M. It usually closes at 6 P.M., although it may be kept open until 11 P.M., if necessary. All lectures, except those intermixed with the drill work of the third undergraduate year, are given in the forenoon. I have not found that particular hours are preferred for research work; though, indeed, the student's day is generally so full that hours are apt to be arranged rather by necessity than from convenience. Every student who works an hour as 'Versuchsobject' for his friend, takes it for granted that the friend will do him a like favour; and so the groups constitute themselves, very amicably. Every leader of a research group has a weekly meeting with the director, to report progress and discuss methods. The number and character of investigations differ considerably from year to year; at present three major and six minor studies are in course.

But the conduct of a laboratory is a personal matter. What I have written so far has, I hope, something of an objective ring about it. When a science is so young as experimental psychology still is, there must, of course, be differences of opinion as regards the indispensable parts of an equipment, the best method of reaching a proposed result, the proportioning of instruction and research. But these are all questions of degree. The question of the conduct of the laboratory at large, as envisaged by the one man responsible for it, is much more important; not only does much more hinge on it, but our answers to it may differ

in kind. I offer my own personal view here with some diffidence, because the subject is not one that lends itself easily to any but conversational and quasi-confidential treatment. I believe, however, that if each of us will state his own conviction, and give reasons for it, we may attain to a far better co-ordination and correlation of our university and college laboratories than at present exists.

For my own part, then, I have always felt that it is very necessary to conduct a laboratory upon the lines of a determinate psychological system. This is one of the chief lessons that I learned, without realising at the time what I was learning, at the Leipsic Institute. In my German student days we all knew whereabouts in Wundt's system we were working, and that our results would reinforce or modify this or that statement of the *Physiologische Psychologie.* The consequence was that we all knew the system ; we had a standard of reference for every new investigation. I believe that this is the way to make one's students psychologise, and I have followed it to the utmost of my power. Even in the drill-course, I do not allow the experiments to remain wholly detached and self-sufficient, as they are, *e.g.*, in Prof. Sanford's *Laboratory Course ;* I try always to hang upon them some suggestion of further problems in a systematic psychology. Still more do the graduates, who have already taken my systematic course, approach their work with a keen eye for general relations. I can see objections to the plan : the laboratory tends to become the expression of a single man's ideas, and the demands upon the time of the director are such as almost entirely to preclude his own accomplishment of original work. But I see much more objection to the opposite plan, of complete freedom in choice of problem and of method ; and compromise is difficult. I may add that, in my experience, the American student is on the average—there are many exceptions—quicker in generalisation and less patient of minute and laborious search for facts than are the English and the German ; and that I have found my plan of work to produce good effects in the way of training and discipline, quite apart from its scientific results. The details of the 'system' may be right or wrong ; that is really a minor matter, provided that the system itself be clear and self-consistent. It is of the nature of a system to be modified under stress of new facts. Advance in scientific thinking always implies an unlearning of old beliefs ; and I do not see that it matters how many things we unlearn, if only we have never to unlearn our method.

I know that these opinions are not generally held; and I should not have intruded them as a part of the *vie intime* of my laboratory did I not hope that their expression will evoke criticism, and so lead to results of practical value. Nothing need be said, in a psychological journal, as to the nature of psychological problems. Students of psychology who are unfamiliar with the laboratory may take it for granted that our problems are the same as theirs. The difficulties that they try to resolve by observant introspection, by the genetic method, by the collection of statistics, we are trying to resolve by an introspection held under experimental control,—to secure which we levy tax upon physics or physiology or chemistry or any other wealthy science that may come our way. That is the only difference.

8. *Student Attendance.*—The times are hardly ripe for a discussion of the function which a department of psychology discharges in the general economy of the university. Laboratory psychology has not yet grown to its full stature; and its youth has been vexed by the interference, friendly and hostile, of older disciplines. Nevertheless, it can do no harm to set down the facts that we have at our disposal, whether they are final or not.

As regards the place of psychology in the undergraduate curriculum, I think we can speak pretty positively. I see no reason at all why, under a system of free election, a course in psychology should not compete on even terms with courses in literature and history, and laboratory work in psychology with laboratory work in physiology or physics or zoology. There can be no doubt of the intrinsic interest of the topics with which psychology deals.

On the other hand, the question of the attractiveness of psychology to the graduate student is much more difficult of decision. It is probable that the graduate attendance at the American laboratories is now at its lowest ebb. A few years ago, when several important laboratories were in course of founding, and instructors were in great demand, the attraction of the new study was, naturally, very considerable. To-day, the places are filled; there is little likelihood of more establishments on a large scale; and the increased number of laboratories means an increased severity of competition. A man who has been fitted by undergraduate courses in history, languages, sociology and psychology to undertake (let us say) either literary work or the research work that shall prepare him for a psychological chair, may therefore very well prefer literature to psychology.

So much on the debit side. As a set-off, we may notice

the following facts. First, the training that can now be obtained in the American laboratories is at least as good a fitting for work in an American university as can be gained in Germany. Hence, while a large percentage of students will doubtless continue to spend a year in Germany, for the sake of acquiring the language and seeing the German equipments, the number of Americans who take the German doctorate will probably decrease, and the number who take the American degree correspondingly increase. Secondly, the establishment of laboratories, while it increases competition, guarantees a psychological career, if only one is capable. Livelihood and reputation are there, to be grasped by the competent. Thirdly, I regard it as probable that the requirements laid upon teachers, in normal and high schools, will in the near future become heavier rather than lighter, and that a good proportion of those who intend to devote their lives to teaching will find it useful to have the psychological degree. Of course, the relation of the teacher to laboratory psychology is extremely complicated. It has been taken as an axiom that pedagogy must be based upon psychology, and teachers and psychologists alike have lost sight both of the difficulty of 'application,' of the translation of theory into practice, and of the immaturity of psychology itself. There has thus been, in the recent past, a provision of crude psychological literature, on the one hand, and on the other a rush for books and keen disappointment at their contents,—a swing of the educational pendulum towards psychology and a reactive swing away from it. But it is surely undeniable that, in the long run, the teacher will gain a great deal from the laboratory: what one may call the psychology of function,—the psychology of attention, of memory-type, of action, of invention,—all this does bear upon teaching. As child-study passes from the unschooled to the schooled, as the bridge is built from anthropometry across individual psychology to experimental psychology, as laboratory psychology ceases to be either an educational fashion or a scapegoat for the sins of past educators: in a word, as the public comes to see things psychological and educational in their right perspective, we may expect that the teachers' colleges and the pedagogical departments will keep a steady stream of students circulating through the psychological laboratory. On the whole, then, I look for a slow but constant increase in the number of our graduate students.

My sketch is now finished. Doubtless, there are many omissions; one is apt not to see the obvious. But the

reader will be able, I hope, to form some notion of the activity of a psychological laboratory, of the nature of a psychological course that is surrounded from beginning to end by the laboratory atmosphere, of the duties that devolve upon the director,—in general, of the 'running' of the whole machine. And he will probably ask: What comes out of it all? It would be hardly fair to conclude my paper without returning some sort of answer to this question.

In the first place, then, training comes out of it. Education, discipline, method come out of it; and that they are of a kind worth paying for is attested by the bare fact that in America they are paid for, that a laboratory equipment is a thing of course at the universities, and is coming to be a thing of course in the colleges. In the second place, contributions to knowledge come out of it. During my six years at Cornell the department has published three major[1] and eighteen minor studies; it has three major[2] and three minor practically ready for publication; and six minor studies are in progress. There is nothing epoch-making about current laboratory work: psychologists have not yet discovered their argon or their Röntgen rays: but it is none the less work, work whose methods get discussed and assimilated, and whose results gradually alter the face of the science. In brief, the two things that come out of the physical or chemical laboratory, precisely those two things come out of the psychological,—trained workers and original work. And when one thinks how great a part of the world of knowledge rests upon psychological foundations, one sees how great is the import of these two things for knowledge, compare them with whatsoever else one will.

[1] M. F. Washburn, "Ueber den Einfluss von Gesichtsassociationen auf die Raumwahrnehmungen der Haut," *Phil. Stud.*, xi. A. J. Hamlin, "Attention and Distraction," *Amer. Journ. of Psych.*, vi. W. B. Pillsbury, "The Reading of Words: a Study in Apperception," *ibid.*, viii.

[2] E. A. Gamble, "The Validity of Weber's Law for Smell Intensities". S. E. Sharp, "The Relation of Individual to Experimental Psychology". I. M. Bentley, "The Qualitative Fidelity of the Memory Image".

III.—THE REGULAE OF DESCARTES. (II.)

(*Conclusion.*)

By Boyce Gibson.

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IV.—*The Four Rules.*

A: Order (1, *Synthesis*).—Of the four rules given in the *Discours*, the third is undoubtedly the most essential; taken in conjunction with the first, which is more of the nature of a preface to the three others than an actual rule itself, it prescribes the order of research which must be observed in order to attain to a sure science of all things. Its leading note is the idea of order, a conception which lands us at once within the very heart of Descartes' method. It would be impossible to exaggerate the importance which Descartes attributed to this observance of order. In Reg. XIV. we read that the whole treatise is concerned with little else than the business of thinking out a subject in an orderly way, and this is borne out by the facts. For whilst Regulae V., VI. and VII., the only rules which the pure reason need observe in preparing a problem for its final solution,[1] are devoted to "stating and unfolding the precepts

[1] Reg. XIII., § 98.

of methodical order";[1] the final solution of a problem itself depends, as we learn in Reg. XVII., on the adoption of a certain direct order of solution, the establishing and the universalising of which is the crowning triumph of the Regulae.

There seems, at first sight, to be a little confusion of thought in Descartes' commentaries on this third rule. He seems to have in mind, now the orderly march of inference in the development of a solution, and now the orderly sequence to be observed in the problems attacked and the studies successively undertaken. In Regulae V. and VI., we have the first of these aspects emphasised; in Reg. IV., § 22, it is the second that is insisted upon. "I have resolved," writes Descartes, "rigidly to observe such order in my search after knowledge as to start always with the easiest and simplest studies, and never to pass forward to others until there seems to be nothing left to be desired in those first undertaken; and it is for this reason that I have so far, to the best of my ability, made a study of this universal mathematic, so much so indeed that I fancy I shall soon be able to handle in their turn the loftier sciences without fear of being premature in my efforts."

It is probably this latter aspect which Descartes had immediately in view when he formulated his four rules in the *Discours*. For after stating in rule No. 3 that the first requisite of order was to start with such objects as were simplest and easiest to grasp, he proceeds a few lines farther down to point out that the demonstrations of the geometers satisfied best these requisites of simplicity and easy grasp, and that he had consequently started his search after truth by a fresh study of those very theorems which the Greeks and others had already proved.[2]

The barrier between these two points of view ceases, however, to exist so soon as we recall Descartes' conception of the unity of all science and the consequences which immediately flow from that conception. At the very threshold of the Regulae we have the great principle of the unity and universality of Science firmly laid down. 'The sciences taken all together are neither more nor less than the human intelligence itself, which is ever one and the same, however different its object-matter;' what have therefore been treated as single, separate sciences, are really 'all involved in each other and are mutually dependent' (§ 1); in fact, they implicate each other to such an extent that Descartes holds

[1] Reg. VIII., § 44. [2] *Cf.* Reg. X.

it 'easier by far to study them all together than to take one in separation from the rest' (§ 1). The direct consequence of this view of the correlation of the sciences is to make the order of scientific inquiry quite independent of the subject-matter considered. This is explicitly brought forward by Descartes himself in one of his letters.[1] 'It should be observed,' he says, alluding to his *Méditations*, 'that in all that I write the order that I follow is dictated, not by the nature of the contents, but solely by the exigencies of the reasoning; in other words, I don't in the least undertake to state in one and the same context the whole of what pertains to the subject I am dealing with. . . . But when I pursue an orderly train of reasoning, *a facilioribus ad difficiliora*, I infer one truth from another, now by means of this subject-matter, now by means of that.' The true order is observed, then, not by saying at any point all that can be said about this or that matter, but by stating all that must be said to justify the inference from one truth to another.[2] The reasoning at each step must be sound and complete, but the vast subject-matter of science may be broken up at pleasure.

We see, then, that the synthetic movement of mind by which Descartes proposed to build up the entire structure of knowledge, presupposed the unity of all Science as synonymous with the unity of Reason itself, and that the order which it observed in its progressive march was in no way conditioned by the special matter with which Reason had to deal. The orderly march of inference in the development of a solution falls therefore into line with the order to be observed in the studies one successively undertakes. For these studies are not distinguished from each other by means of their subject-matter, but only by means of their relative complexity. The march of inference and the order of study are both advances from the simple to the more complex in a sense which is precisely the same in the second case as in the first. The complexity which Descartes invariably has in mind is in fact a complexity of a purely logical kind. The most complex phenomena of all are but complicated entanglements of a few ultimate and absolute truths. Truth is of one nature, and Knowledge of one structure through-

[1] V.C., viii., 431.

[2] Thus in the '*Abrégé*' which Descartes gives immediately after the Preface to his *Méditations*, he says: 'I have found myself obliged to follow an order similar to that adopted by geometers, which consists in first bringing forward all the premises necessary for the establishing of a proposition before drawing any conclusion from these same premises'.

out, 'consisting solely in the combinations of irreducible elements'.[1] Thus it is, that whenever we deduce an unknown fact from facts previously known, we do not on that account light on some new species of being. All we do is so to extend our previous knowledge as to perceive that the thing we seek shares, in one way or another, the proportions of those objects which are given to us in the proposition itself.[2] Thus we need not imagine, in the case of the magnet, that its action can only be explained by bringing in some new order of being foreign to all our previous experience. 'He who reflects that there is nothing we can know in the magnet which is not made up of certain simple, obvious elements, will have no doubt as to how to proceed, but will first diligently collect all the well-attested facts he can muster concerning the nature of this stone, and will then endeavour to infer from these that commingling of simple elements required to produce all those effects which have been observed in the magnet.'[3]

This conviction that science can recognise no difference in nature between one truth and another, but only a difference of complexity, is fundamental with Descartes. There is identity of nature between the relative and the absolute, or at least the relative shares the nature of the absolute sufficiently to constitute a connexion by means of a chain of deductive inference. 'The entire difference,' in fine, 'between one truth and another, lies solely in the distance we must cover to reach them.'[4]

The order of inquiry, prescribed in the third of the four rules of the *Discours*, is, from either of the two points of the view we have been discussing, the order to be followed by such as are seeking to find out the truth of things, and should not be confused[5] with the order most suited to the exposition or explanation of what one has discovered. In this task of explaining to others what one already knows[6] Descartes allows that one can find a certain help in the rules of syllogism. But this is a mere concession. Descartes himself does not resort to syllogisms when in his *Essais* and *Principes* he endeavours to explain his Method and his System.[7] He simply allows himself a certain freedom in variously rearranging the order of discovery so as to adapt it the better to the purposes of exposition. Occasionally the

[1] Reg. XII., § 94. [2] *Cf.* Reg. XIV., § 110.
[3] Regulae, § 96. [4] *Ibid.*, § 57. [5] See V.C., vii., 377.
[6] *Cf. Discours*, part ii., § 6, and the closing words of Reg. X.
[7] *Cf.* V.C., viii., 438; ix., 24.

two orders coincide. This is notably the case in Descartes' *Géométrie*,[1] and, as Descartes himself points out,[2] in his treatment of the rainbow in the *Meteors*. Finally, we should notice that in the concluding words of the third rule, a distinction is made between an order that is given in the very nature of the question to be examined and an order that must be introduced into the problem, as it were from the outside. Where there is a certain obvious order of treatment prescribed, that order must be emphatically followed. It is what Descartes terms the 'natural' order. 'I have now grown so bold,' he writes on one occasion,[3] 'that I am now venturing to find out why it is that such and such a star is in such and such a place: for though they appear to be scattered at random over the sky, still I have no doubt but that there is a natural order in their arrangement which is regular and determinate.' In another letter,[4] Descartes suggests that the key to a universal language might be found by introducing among all the ideas which the human mind is capable of conceiving an order similar to that which is found naturally to exist among numbers.

It may often happen however that this natural order is not inherent in the thing itself. In this case it must be cunningly ingrafted.[5] 'Thus,' says Descartes, 'if we wish to make out some hidden cipher, written in unfamiliar characters, there is indeed no obvious order in the thing itself, but we must assume a certain arrangement.'[6] This ingrafted order, as we subsequently learn, must in every case be methodically devised and so arranged as to obviate any risk of going twice over the same ground.[7]

The principle of Economy, emphasised in the words we have just quoted, is in Descartes' eyes essential to methodical order, and receives the fullest recognition in the Regulae. Again and again, especially in the latter half of this treatise, we are explicitly urged to respect the gift of reason by a rigid economy of its resources, scrupulously avoiding every superfluity. It is here that we find a distinct lacuna in the four rules of the *Discours*.[8] No allusion is made there to the need of mental economy or to the evil of wasting time on

[1] *Cf.* 'La *Géométrie* de Descartes au point de vue de sa méthode,' *Revue de Métaphysique et de Morale*, Juillet, 1896.

[2] *Cf.* V.C., vii., 377. [3] V.C., vi., 209. [4] *Ibid.*, vi., 66.

[5] *Cf.* Reg. X., § 63. [6] Regulae, § 63. [7] *Cf.* Regulae, § 42; also § 17.

[8] We shall have occasion to notice a second when we come to consider Descartes' conception of Analysis.

the unessential; in the Regulae such allusions abound. Rules XIII., XIV., XVI., XVIII. and XX. breathe, one and all, this spirit of economy, and betray, in the very terms in which they are enunciated, a distaste of the superfluous.

Rule XIII. treats of the way in which we should handle the problems we desire to solve. We must take care, in the first place, not to assume more than our data give us, nor yet to interpret these data too restrictedly (§ 106); we must in fact 'see exactly wherein the difficulty consists, in order that, disembarrassed of all its accessories, it may be the more easily solved'. Thus in the case of the vase of Tantalus (§ 108), 'the difficulty stripped of its accessories is reduced to the inquiry as to why all the water previously in the vase flows bodily out as soon as a certain height is reached'.

In Rule XIV. this process of abstraction in the handling of problems is shown to culminate in their reduction to purely quantitative form; and we read farther on (§ 125) that 'it is no less necessary to abstract from a proposition those figures which geometers treat of, supposing the question to refer to these figures, than it is to abstract from it any other matter whatsoever'. 'The real extension of bodies must be consistently set before the imagination by means of the barest figures, for when so depicted, it is far more distinctly understood by the mind.'

Rule XVI., following up the directions of § 109, explains a device by means of which 'we not only economise our words, but, what is of special importance, present the difficulty in terms so transparent and abstract that, whilst we omit nothing that is useful, we yet leave nothing that is superfluous and which might fruitlessly take up the attention of our mind when it was requiring to grasp several things at once' (§ 128). This device is only an application of the principle of economy to the question of Mathematical Notation. Lastly, Rules XVIII. and XX. require that the same principle should be observed in all operations of a mathematical kind.

This principle of economy in method has its counterpart in the law of least action in Nature herself. To what extent Descartes realised this it would be difficult to say. But his great rival Fermat, writing two years after the death of Descartes to M. de la Chambre, concerning the problem of refraction, says: 'The proof I give is based on this one postulate — *Naturam per vias breviores operari—i.e.*, that Nature adopts the easiest and promptest means to reach her ends; for it is in this sense, and not, as many put it, that

Nature always takes the line that is the shortest, that I consider that this axiom should be understood'.

A : Order (2, *Analysis*).—Having studied Descartes' conception of synthesis in connexion with the third rule of the *Discours*, it is natural that we should next turn our attention to his conception of Analysis.

The first definite statement as to what Descartes understood by Analysis is to be found in the enunciation of Reg. V. It is a gradual reference of propositions that are involved and obscure to simpler propositions. In the commentary that follows, Descartes makes it quite plain that his essential conception of scientific analysis is that of an investigation that proceeds step by step. Any such investigation which is not deliberately constructive, and therefore synthetic, constitutes an Analysis from Descartes' point of view. At the same time such analysis has a goal as well as a method. Its aim is to 'make diligent search for what is most absolute'.[1] This search may take the form either of a gradually receding reference from the more to the less relative—of which we shall speak later—or the form of a rigid deduction.

When considering analysis as a mere propædeutic, Descartes treats it as a form of pure deduction. Before one can be a master of analysis, in the sense of being able to refer the relative swiftly though gradually back to the absolute, one must have made oneself familiar with a number of main chains of logical dependence between the complex and the simple, so as to be able eventually to mould every question requiring solution into the form of one or other of these 'chains of inquiry'.[2] Descartes has accordingly the following advice to offer to the young student: 'Collect at haphazard the very first truths that present themselves and then see, one step at a time, whether others cannot be deduced from these, and others again from those just deduced, and so on in succession, the object of this study being to gradually establish in the mind such an organisation of relatives and absolutes, that, whenever we do attack some definite question, we shall be able to judge what previous discoveries will best repay a preliminary study'.[3]

Here analysis takes the form of deduction, but once we are concerned, not with the mere practice of method, but in the actual solution of problems, analysis ceases to be a deductive process: it becomes a process of regressive reference

[1] Regulae, § 29. [2] *Ibid.*, § 30. [3] *Ibid.*, § 31.

in the direction of the absolute factors in the problem, the deductive work being now relegated wholly to the reconstructive work of synthesis as expressed in the third of the four rules of the *Discours*.

This aspect of analysis as mere regressive reference is the one in Descartes' mind when he is enunciating Reg. V., and is admirably illustrated in Descartes' own discussion of the anaclastic, as given in Regula VIII., § 47. The problem being to seek a line from which rays impinging in a parallel direction are so refracted as to meet at one and the same point after refraction, the investigator starts this process of regressive reference when he notices that the ratio between the angles of incidence and refraction is bound up with the changes it undergoes as the media vary; and is carried successively farther as he sees that the amount of this change depends in turn on the way in which the ray travels through the whole transparent medium, that his insight into the nature of this penetration presupposes a knowledge of the nature of light; and, finally, that to understand what light itself is, it is necessary to know what is meant in general by a power of nature, the last term and the most absolute in all this series. Here the regression ends, and the synthesis or reconstruction begins on the lines marked out in Regula V. The investigator starts afresh from the intuition of what is meant by a power of nature, and then works his way deductively forward to a knowledge of the anaclastic by means of the very steps that helped him down to his starting-point.

In the third of the four rules of the *Discours* where Descartes enunciates his rule of synthesis, he makes no allusion to the regressive analysis that, according to Reg. V., is its indispensable antecedent. This is no serious omission. Nor should we expect him to mention those deductive analyses which, as we have already seen, were simply of the nature of a propædeutic. There is, however, a definite and a final form which Descartes' conception of analysis took in the Regulae, the omission of which from the rules of the *Discours* makes a distinct gap there in Descartes' conception of Method.

In the advice which Descartes gives to the novice in Analysis, he recommends him to collect at haphazard certain truths, and deduce as many further truths from them as he can. He is to argue 'That is true, therefore this is true'. This is, however, only a training-ground for the far more general form of argument: 'If that is true, then this is also true'. Here analysis starts no longer from what is

obviously true, or has been proved to be true, but only from what is assumed to be true. The method of analysis becomes thus in its most final and scientific form a method of proceeding step by step from the unknown to the known, on the assumption that the unknown is already known. This method, the exposition of which forms one of the main features of the Regulae, is fully explained in Reg. XVII.: 'The difficulty in question,' says Descartes, 'must be investigated in a straightforward manner. We must disregard the fact that, of its terms, some are known and others are unknown, and keep to a true chain of reasoning, our eyes fixed solely on the mutual dependence of the various terms.' Every solution of an equation is an illustration of this method, and the mere setting of it an embodiment of the principles on which this method relies. The equation is in fact the form to which Descartes himself is driven by the method he adopts. 'In accordance with this method of inquiry,' he says, 'we must find out as many magnitudes, as we have unknown quantities treated as known for the sake of adopting a direct method of solution, and express each of them in two different ways. This will give us as many equations as there are unknowns.'[1] By the adoption of this method, Descartes cancels the two indirect methods of analysis to which he refers in Reg. VI.[2] The direct method has been generalised so as to cover all possible cases,[3] developed, *i.e.*, into one uniform method of analysis.

B: Distinctness.—It has not unfrequently been considered that Descartes' reference to Analysis in the four rules of the *Discours* should be identified with the injunction of Rule No. 2. In a certain sense it is no doubt true that to split up a difficulty is to analyse it, but so understood, analysis ceases to possess that definite meaning which in Descartes' mind is quite essential to it. As we saw, an analysis is in every case a process of getting, step by step, into the heart of a subject: where there is no gradual progress of this kind, there is no analysis in the true sense of the word.

There is one point, indeed, at which the results of such analysis seem to coincide with the results of obeying the injunction given in Rule No. 2. Referring to the supreme usefulness of the precept embodied in Regula VI., Descartes says: 'It points out to us how all things can be split up into certain groups, not indeed on any ontological basis such as philosophers adopt in the use of their categories, but on one of logical dependence, such that whenever a fact

[1] Regula XIX. [2] Reg. VI., §§ 32, 33. [3] *Cf.* Reg., § 134.

presents a difficulty, we can at once perceive whether a preliminary investigation of certain other facts will be likely to prove helpful, and if so, which, and in what order'. The analysis which proceeds link by link ends in riveting within the mind a number of different chains of inquiry, each of which has all its links logically subordinated to one another. Facts are thus split up into certain groups and, so far, analysis seems to be the method prescribed in Rule No. 2; but the likeness is quite superficial. Once we recollect that the groups referred to represent, in the one case, 'different chains of inquiry' into line with some one of which every question should at the outset be brought; and, in the other, definite divisions within the limits of the question itself, we see that this latter process of division is quite another thing from the gradual establishing, through a process of repeated analysis, of a number of logical chains between relatives and their respective absolutes. The analytic method of Reg. VI. teaches us how to assign a difficulty to its proper place in the hierarchy of relatives and absolutes; the precept of the second rule has quite another end in view. Its main object is to simplify a difficulty by studying it successively in its parts instead of simultaneously as one whole. In Reg. XIV., § 119, we have this clearly brought out. We shall suppose that a certain problem relative to a tetrahedron has been set for solution. Now in the case of a tetrahedron, the data must consist of at least six different elements, or the problem is insoluble. But it would be eminently unmethodical to attempt to deal with these six elements all at once. The art of the inquiry must rather consist 'in splitting up these data into as many groups as possible in such a way that though we include all these groups successively in our survey, we need never attend to more than a very few at a time'. A further illustration of the precept of Rule No. 2 may be found in Reg. VI., § 32,[1] where a complex process of inference is simplified at the outset by subdividing it into two simpler processes.

C: Completeness.—This subdivision of a difficulty into its component parts is obviously a very similar process to that inculcated in the fourth rule, where Descartes is talking of enumeration. The aim of the two rules is, however, quite different. In the second, the aim of subdivision is to facilitate the solution by securing *distinctness* of treatment, whilst the aim of enumeration in the fourth is to guard

[1] *Cf.* Reg. XI., § 70.

against the possibility of omission by ensuring *completeness* at every step. The second and fourth rules represent the requirements of distinctness and completeness respectively, just as the first insists on certainty and the third on methodical order.

It is true that in his treatment of enumeration in Reg. VII. Descartes does refer to it as a principle of methodical arrangement, and in so far as he does so, it would be idle to attempt to draw a distinction between subdivision and enumeration. As we have seen,[1] Descartes' definition of enumeration in the Regulae is purposely of a very vague and general nature, and there is no evidence to show that, had he condensed his method into four short rules at the time he wrote the Regulae, he would have had a second rule at all: the second would probably have been included in the fourth. This confusion of the two aspects of distinctness and completeness constitutes another mark of difference between the Regulae and their counterpart in the *Discours*.

We may then take such sections as the forty-first and forty-second, in Reg. VII.,[2] as representing the nature of that common source from which the second and the fourth rules of the *Discours* took their rise. Here the main emphasis is laid on the necessity of arranging the elements of a problem in perfect methodical order. So arranged, they will be seen to distribute themselves under certain heads, as Rule No. 2 requires, and must be carefully reckoned up in accordance with No. 4.

As regards this fourth rule little need be said, as it deals solely with enumeration, and we have treated of enumeration elsewhere. There are, however, just one or two points in its enunciation to which it may be well to draw attention. In the first place, the 'complete enumerations' and 'general revisions' to which it separately refers should not be confused with each other or taken as synonymous. The enumeration which Descartes has especially in mind in enunciating this fourth rule is, as we see a couple of pages farther on, an exact summing up of all the conditions on which the solution of a problem depends, a process which should take place at the outset of an inquiry, whereas revision comes naturally at the end.

In the second place, it is interesting to notice that in a Latin translation of the *Discours*, published in 1644 and carefully revised by Descartes, Descartes has added the following

[1] *Cf.* Reg. VII., § 38.

[2] *Cf.* also the Enunciation of Reg. XIII.

words to the enunciation of the fourth rule: 'tum in quærendis mediis, tum in difficultatum partibus percurrendis,' so that the revised rule would read as follows: 'I must make at every turn, whether I be seeking methods of solution or running over the various parts of a difficulty, enumerations so exhaustive and revisions so general in kind, that I may be sure of having omitted nothing'. The translation of the words 'in quærendis mediis' which I have ventured to give[1] is based on a certain use of the word 'medium'[2] adopted by Descartes in a letter to Mersenne.[3] He is indignant at Roberval's criticisms of his proofs of the roulette, and after protesting against the accusation of having shifted from one method of solution (medium) to another in the course of the demonstration, adds: 'He ought to be ashamed of boasting that he has a method (medium) for finding the tangents of the roulette which can be applied to all cases'.

Distinctness as a Condition of Certainty.—In commenting upon the second rule, we saw that it was quite misleading to connect the second and third rules together as representing the Cartesian methods of analysis and synthesis respectively. Far indeed from connecting itself naturally with the third rule, the second follows a great deal more logically from the first. The solution of any and every problem is possible only when each successive step in the solution is intuitively grasped. The best way, then, of ensuring an easy and speedy solution is to facilitate this work of intuition. It is Descartes' conviction that this end is best secured by splitting up the problem to be dealt with into as many parts or departments as possible. The first rule insists on the need of intuitively grasping each element in the solution of a problem, be it a datum or some step in the proof, and the second points out the one preliminary way by which this need can be satisfied. To grasp a point intuitively, that point must first be isolated, but to isolate the various points in a difficulty is to split it up into as many parts as its nature allows. That this is in all probability what Descartes has in mind when he places the second rule immediately after the first, follows from a number of passages in the Regulae, all of which emphasise the fact that intuition requires and involves separation of the elements to be intuited.

[1] I cannot agree with M. Aimé-Martin's translation (*Descartes*, second edition, Paris, 1844). 'Soit en cherchant le centre des choses.'

[2] Descartes' usual use of the term is of course quite orthodox. *Cf.* V.C., viii., 85, 364.

[3] *Ibid.*, vii., 455.

The ninth Regula is solely occupied with showing that if we would accustom ourselves to see truth clearly and distinctly we must rivet our thought persistently on the very simplest elements we are able to discover. 'He who wishes,' says Descartes, 'to take in a large number of objects at one and the same glance sees none of these distinctly,' and similarly, 'he who is wont in one and the same act of thought to divide his attention among many objects, is left with a mind all in confusion';[1] 'we should therefore accustom ourselves,' he continues a little farther on[2]—and here the link connecting the first and second rules is plain to see—'to hold in our thought so few things at a time and things of so simple a kind, that we shall never imagine that we know anything which we do not grasp with a distinctness equal to that with which we apprehend the most distinct things of all'. The same warning applies to these distinctest of all things, the simple, ultimate elements: to grasp them intuitively 'we have only to separate them off from each other and consider each, in succession, with riveted attention'.[3] Finally, in solving any proposition, the art of the inquiry 'consists in splitting up the elements or dimensions of the problem into as many groups as possible, in such a way that though we include all these groups successively in our survey, we need never attend to more than a very few at a time'.[4]

D: Certainty.—Let us turn now to the first rule itself: 'Never to admit the truth of anything which we do not clearly recognise as true, *i.e.*, to studiously avoid any haste or prejudice, and to include in our judgments nothing which does not present itself so clearly and so distinctly to our minds, that we have no reason whatever for doubting it'; or, in the corresponding words of the Regulae: 'Never to busy ourselves with anything about which we are unable to reach a certainty on a par with that of arithmetical and geometrical conclusions (§ 7), to avoid presupposing data little understood or passing hasty and ungrounded judgments (§ 5), and to introduce no conjectures whatsoever into our judgments concerning the truth of things (§ 10)'.

The test of truth with Descartes is a certainty of the reason, springing, positively, from the very structure of reason itself, and negatively attested by the absence of any conceivable cause of uncertainty. In his enunciation of this rule concerning the test of truth, Descartes explains

[1] Reg., § 56. [2] *Ibid.*, § 58. [3] *Ibid.*, § 92.
[4] *Cf.* also Reg., § 127 and end of § 119.

the positive aspect by means of the negative. It is by repressing every tendency to think impatiently or judge hastily, by putting gradually aside all one's preconceived ideas, and by confronting and overcoming every possible objection that could conceivably be raised, that the mind comes at length to see things clearly and distinctly. For, as we have already pointed out, the criterion with Descartes is essentially the natural light of the reason, a light which can only shine out truly when unobstructed by prejudices, and undisturbed by 'ill-regulated studies and confused reflexions'.[1] 'We can add nothing to the pure light of reason which does not more or less obscure it.'[2] This negative aspect, in the work of securing a trustworthy standard of truth, is clearly emphasised by Descartes in one of his letters.[3] Referring to the confessedly unsatisfactory nature of his proof of God's existence, as given in the *Discours*, he says: 'It is quite impossible to realise the certainty and the clearness of the reasons which go to prove the existence of God, unless one is able distinctly to recall those which point out the uncertainty of all that we presume to know about material things'.[4]

There are one or two further passages in Descartes' correspondence which emphasise the positive aspect of the test of truth. Once a methodical process of doubt has relieved the mind of its prejudices and obscurities, and left a way open for the natural light to pour through, and shine straight upon its object, there is no further need to ask, 'What is truth?' The question is quite irrelevant, 'for there would be no means available for learning what truth was, did we not know it by nature';[5] 'our soul,' he writes elsewhere, 'is by nature such that it must needs accept [as true] what it distinctly conceives [to be true]'.[6] Clearness of Insight or Truth depends thus in last resort on clearness of soul. If it is the clear soul that is speaking, it is truth that is being uttered. Truth is an instinct of this clear soul. 'As for me,' says Descartes, 'I distinguish between two kinds of instinct; one belongs to us in our capacity as men, and is of a purely intellectual nature: this is the natural light or "*intuitus mentis*" on which I hold that we should alone rely; the other we possess in our capacity as animals, and it consists in a certain natural impulsion. . . .'[7] This intellectual instinct or natural light Descartes explicitly accepts as his "*règle des vérités*" or criterion of truth.

[1] Reg., § 17. [2] *Ibid.*, § 18. [3] V.C., vii., 378.
[4] *Cf.* also V.C., viii., 57. [5] *Ibid.*, viii., 168.
[6] *Ibid.*, viii., 220. [7] *Ibid.*, viii., 168.

In the same interesting and instructive letter,[1] Descartes has occasion to remark that all men have one and the same natural light. This is a central doctrine of Descartes. A thing is not to be accepted as true because a universal consensus of opinion declares it to be true; but, if a fact is true, and the intellectual instinct is allowed in every soul its unobstructed expression, there will be at once perfect agreement among all minds. The *intuitus mentis* is the universal soul of mankind.

The Regulae abound with passages that illustrate this democratic conception of the fundamental nature of Reason, and the very first words of the *Discours* repeat the same conviction that the power to pass valid judgments and to distinguish what is true from what is false . . . is by nature equal in all men. It is for this reason that 'deduction can never be wrongly made, even by the least rational of minds,'[2] for deduction is only this intellectual instinct in movement. It must not be supposed, however, that Descartes made no distinction in mental capacity between one man and another. He admitted that some minds work far more rapidly than others,[3] that Nature had not endowed all men with an equal degree of originality,[4] and that in a certain sense one man might be said to be less defective in natural capacity than another.[5] But only in a subordinate sense. Differences in originality, promptitude and acuteness, are only accidental, and mark no essential or natural difference between man and man.[6] Even allowing that these accidental differences are more or less ineradicable, they do not affect the essential quality of the mind, but only the ease and success of its application. Descartes' aim throughout the Regulae is consequently to expound a method which, if scrupulously followed by any person, however devoid he might be of accidental advantages, must needs bring him eventually to the same high level of Knowledge as is open to the acutest and most original of thinkers. 'We shall endeavour throughout this entire treatise,' says Descartes,[7] 'to treat so accurately of all the ways which are available to men in their search after truth, and to make these ways so simple, that however ordinary a man's natural capacity may be, provided only that he be thoroughly imbued with this whole method, he shall come to see that there is no truth

[1] V.C., viii., 168. [2] Reg. II., 5. *Cf.* also Reg., §§ 5, 12
[3] *Discours de la Méthode*, 1ière partie, § 2.
[4] Reg. X., § 62. [5] *Ibid.*, § 79. *Cf.* also Reg., § 58.
[6] See *Discours*, 1ière partie, § 2. [7] Reg., § 54.

hid from him from which everybody else is not also precluded.' He will come fully to perceive 'That any ignorance he may have is due neither to want of natural capacity nor to lack of skill, and that there is positively nothing which another can know and yet he himself be incapable of grasping, provided only that he gives it suitable attention'.[1] Method and the will to follow it in every particular is thus all that is needed to render the completed science accessible to the least endowed of intellects, for the method provides 'that in no instance does the tax on one's natural ability exceed that which is imposed by the drawing of the simplest inferences,' and these simple deductions can never be wrongly made by any one.

Liberty and Leisure.—The first of the four rules of the *Discours* has, as we have seen, a general and prefatory character. It states the criterion of the method, the standard by which false is distinguished from true. In the manner in which this statement is made, there is enjoined a certain attitude of mind in the practice of the method which it may be of interest to notice. There must be no hurry, we are told, and no prejudice. The true philosopher's motto, in other words, should be 'Liberty and Leisure'. It may well be said to have been the motto of Descartes himself. In a letter to one of his friends,[2] he writes: 'I fear fame more than I desire it, as, in my opinion, it invariably interferes with the liberty and the leisure of such as acquire it. I enjoy these two advantages so fully and value them so highly that there is no monarch rich enough to purchase them from me;' and he adds 'Bene vixit bene qui latuit'. We are reminded of 'the freeman, who has been trained in liberty and leisure and who is called the philosopher';[3] and of Edgar's advice to Gloster in "King Lear": 'Bear free and patient thoughts'. This then is the spirit of the first of Descartes' rules: our thinking, to be true thinking, must be both free and patient. The standard of truth is in thought itself, but thought is only itself when it has liberty and leisure.

The Subjective Character of Certainty.—The purely subjective character of Descartes' criterion of truth has not recommended it to modern ideas. It has been asserted, and with justice, that no intuition *per se* has, theoretically, a right to be convincing; that the criterion of truth is conformity with the whole system of organised science, and that the part is

[1] Reg., § 48. [2] V.C., vi., 100.

[3] Plato, *Theaetetus* (Jowett's Translation, vol. iv., p. 234).

true only when it is an expression of the truth of the whole to which it belongs. To discuss the general question of the criterion of truth would, however, lead us too far. We will, therefore, content ourselves with giving one example of Descartes' use of his own criterion which may serve to suggest the limits beyond which it is hardly to be trusted. Descartes had been considering the question as to whether light was instantaneous or not, and had come to the conclusion that it was absolutely instantaneous. He based this conclusion on the certainty of his own mind, a certainty such that he declared himself ready to confess, in case any one should prove him to be wrong, that he knew nothing in Philosophy whatsoever.[1]

Descartes' conviction in this case rested in last resort on the fact that astronomical observation had proved the passage of light from the moon to the earth to be instantaneous. The faulty inference makes it only too plain that a purely subjective standard of truth, even when wielded by the most cautious and enlightened of minds, is not sufficiently delicate to gauge the measureless possibilities of Nature.

V.—*The Unity of the Natural Method: Its Mathematical Character.*

In discussing as we have done the nature and meaning of the four famous rules of the *Discours*, we have tried to see into the detail of Descartes' method and to appreciate it from Descartes' own peculiar point of view. Our next duty will be to try and grasp as a whole what we have so far been treating of in parts, and so restore to our conception of Descartes' method that sense of unity which is so eminently characteristic of it.

The unity of Descartes' method is peculiarly easy to grasp on account of its simplicity. With Descartes there is no question of a method that shall adapt itself to the material it is destined to organise; his method is consequently both uniform and universal. Method is one, to Descartes, in the same sense that Science is one: in each case, the unity depends solely on the fact that there is only one way in which the mind can work so as to secure the maximum certainty at each step of its progress. As Science must above all things be certain, this one way—the way, that is, of the geometer and arithmetician—must therefore be universally adopted. Descartes secures the unity of his method by

[1] V.C., vi., 264.

universalising the uniform process of mathematical reasoning so that it may be 'sufficiently comprehensive to draw out the truth from any subject whatsoever'.[1] Referring to his method, as explained in Reg. XIII., he writes:[2] 'I do not hesitate to say that this portion of our method was not devised for the sake of solving mathematical problems, but rather that Mathematics must be learnt almost solely as a training towards the practice of this method'. His method is therefore one, in the sense that it is one and the same in all departments of scientific thinking: it is the method of a universal Mathematic.

Science with Descartes is, in fact, synonymous with a mathematical explanation of the universe. 'Apud me,' he writes in one of his letters, 'omnia sunt mathematice in Natura;'[3] and again: 'Mathematics is the principal basis on which all my reasonings rest'.[4] In another letter,[5] he asserts that the main reason why he holds his own views to be superior to any other lies in his having adopted a kind of philosophising which cancels as invalid any reason which is not mathematical and clear.[6] 'I am of opinion,' he writes, 'that nothing further can be desired in the case of philosophical theorems than the power to prove them mathematically.'[7] We have then in Descartes one who by temperament and conviction is *par excellence* a mathematician, whose whole philosophy is indeed but an expansion of the mathematics by which his method is inspired.

VI.—*The Natural Method: Its Forms of Proof:* A priori *and* A posteriori.

The mathematical attitude which Descartes assumes towards the problems of experience determined the direction in which his method moved. This direction he describes as '*a priori*,' proceeding, *i.e.*, from causes to effect, proving the latter by means of the former.[8] It involves the passage from the absolute to the relative, from the simple to the complex, from the axioms and definitions of Philosophy to the theorems which are founded upon them. A theorem is proved *a priori* when it has been deduced from self-evident principles. Thus from these fundamental truths, that man's composite nature is essentially corruptible, and that his mind is, on the other hand, incorruptible and immortal,

[1] Reg. IV., § 19. [2] *Ibid.*, § 114. [3] V.C., viii., 205. [4] *Ibid.*, ix., 15.
[5] *Ibid.*, vi., 348. [6] *Cf.* also V.C., vii. 121, 434; viii., 123.
[7] *Ibid.*, viii., 336. [8] *Ibid.*, viii., 267, 599.

Descartes proves *a priori* that it is hopeless to expect the same certainty in dealing with what concerns the conduct of life as must be looked for in the treatment of scientific questions.[1]

This *a priori* method is the ideal after which Descartes consistently strives. He calls it 'the noblest possible form of proof,'[2] and would like to see it exclusively adopted. Speaking of a certain natural order which he believes to hold among the relative positions of the scattered stars in space, he curiously adds: 'The knowledge of this order is the key and foundation of the highest and most perfect science that men can possess concerning material things, since by its means we could learn to know *a priori* all the various forms and essences of terrestrial bodies, whilst without it we must be content to guess at these *a posteriori* through their effects'.

The *a posteriori* form of proof which Descartes here contrasts so unfavourably with the *a priori* form is that which attempts to prove an assumed fact by means of the consequences which would necessarily follow, were the fact really true.[3] It would seem to follow from this that an *a posteriori* proof was of that kind which justifies a hypothesis, *quâ* hypothesis, on the ground that Nature herself justifies all the consequences which flow from it, but that, *quâ* hypothesis, it could never rank in validity with the final certainty of an *a priori* proof.

Much that Descartes says supports this natural view of the difference in validity between the two forms of proof. He is usually apologetic when referring to his *a posteriori* proofs,[4] admits that there is some reason for treating them as merely hypothetical,[5] and is inclined to insist simply on the moral certainty of assumptions eminently fruitful in consequences,[6] and on the cumulative force of this fruitfulness. 'All these things are indeed only probable when taken separately, and cannot wholly convince us, but when taken all together they acquire the force of a genuine demonstration.'[7]

But this apologetic tone is not consistently maintained. We have only to look more closely to see that Descartes claims more for an *a posteriori* conclusion than

[1] V.C., viii., 267.

[2] *Ibid.*, vii., 10, where he opposes it to the *reductio ad absurdum*, the least worthy of all.

[3] *Ibid.*, viii., 267.

[4] *Cf.* V.C., vi., 363; vii., 380; ix., 351.

[5] *Ibid.*, ix., 193.

[6] *Ibid.*, ix., 351.

[7] *Ibid.*, vi., 352.

the term 'hypothesis' could possibly supply. In one sense, Descartes does look upon a result reached *a posteriori* as provisional, and in so far as it is provisional it might be looked upon as hypothetical in character. The epithet 'provisional' attaches to it, however, not *quâ* result, but *quâ* result reached *a posteriori*. There is, therefore, nothing hypothetical about the conclusion itself; what is provisional is merely the form of the reasoning that supports it. *A posteriori* reasoning is a mere scaffolding doomed eventually to give place to the more solid substructure of an *a priori* proof. Thus there is a marked contrast between Descartes' *a posteriori* conclusion and the working hypothesis of modern science. The former is permanent *quâ* result, provisional *quâ* method; the latter is permanent *quâ* method, provisional *quâ* result. But, in the second place, not only does Descartes look upon his *a posteriori* conclusions as established certainties of science, but the provisional *a posteriori* method itself is defended as a perfectly rigorous method of deduction, so much so, indeed, that one is inclined to wonder why Descartes should think it necessary to subordinate its validity to that secured by the noble *a priori* method. There is perhaps the same distinction in Descartes' mind between the validity of *a posteriori* and *a priori* proofs as subsisted in Kant's between the value of the '*summum bonum*' and the '*bonum consummatum*'. In both cases the distinction is between superlatives rather than comparatives, between two varieties of 'most' rather than between the more and the less.

An *a posteriori* proof is, therefore, in Descartes' opinion, as truely deductive in essence as an *a priori* demonstration. This we proceed to explain. Descartes was well aware of the impossibility of reasoning backwards from consequence to ground in the same way as one reasons forward from ground to consequence. In the Regulae,[1] we read: 'The converses of many necessary inferences are contingent: thus, from the fact that I exist, I can conclude with certainty that God exists, but I cannot affirm that because God exists, therefore I must also exist'. In the *Discours*,[2] Descartes expresses himself still more clearly and pointedly. 'I must needs confess,' he writes, 'that the potency of nature is so vast and so extensive, its principles so simple and so general, that I can scarce find a single individual effect which I do not see to be deducible from these principles in many different ways; my greatest difficulty, indeed, consists as a rule in

[1] Reg. XII., § 86. [2] 6ième partie, § 3.

finding out along which of these ways the dependence really lies.' It is clear then that the *a posteriori* method presents a difficulty which is absent from the *a priori* form. In so far as this difficulty is unsurmounted or insurmountable, the latter method excels the former in rigour and certainty. But with Descartes the difficulty is far from insurmountable. A few experiments are all that he requires in order to attach a certain effect to its natural cause, experiments 'such that their results differ with the explanatory cause which is assumed'.[1] Descartes seems to argue as follows: The effect E must proceed from one or other of the causes C_1 C_2 . . . C_n, where n is a definite number, say 3. The results of certain experiments show that it cannot be from C_1 or C_2 that the effect proceeds. It must therefore proceed from C_3. Assuming the experiments to be available, the validity of the reasoning depends wholly on our being able to fix a limit to the number of possible causes whence the effect E could proceed. Descartes seems, as a rule, to have been able to do so. Referring to a certain assumption at the opening of his *Meteors*, he says: 'I could not prove it *a priori* without revealing my whole theory of physics, but the results which I have logically deduced from them, and which cannot be similarly deduced from any other principles, seem to prove it sufficiently *a posteriori*'.[2] So again he asserts that he has given more than 500 reasons in his *Dioptrics* and *Meteors* which prove a certain assumption *a posteriori*; '*i.e.*,' he goes on, '500 things I explain by means of the assumption, nor could these be explained without it'.[3] In all these cases the effect is represented as having been traced *a posteriori* to the one cause whence it actually emanates.

To be strictly deductive however, deductive, *i.e.*, in the Cartesian sense of the word, a proof must not only proceed rigorously from one fact to another which is necessarily connected with it, but the starting-point of the inference must be a fact that the pure reason grasps as certain and indisputable. As a rule this starting-point in an *a posteriori* demonstration, is some well-sifted fact of sense-perception, a fact which reason, working through the senses, is able to apprehend intuitively as something which is rationally certain and unmistakable, a fact which, in virtue of its absoluteness and simplicity, is entitled to serve as basis for a rigid deductive proof.[4] Descartes gives a good instance of this aspect of deduction in a letter to M. Plempius.[5] 'The

[1] *Discours*, 6ième partie, § 3. [2] V.C., vii., 380.
[3] *Ibid.*, ix., 351. [4] Reg. VIII., § 46. [5] V.C., vi., 363-4.

things I bring forward in the earlier chapters,' he writes, 'concerning the nature of light, the form of salt-particles, and the like, are not my principles, as your objections imply; they are rather conclusions which are proved by all the facts which follow them. . . . Thus, having taken for granted that salt-particles are somewhat long in shape and inflexible, I deduced the square form proper to grains of salt, and many other things besides which are very obvious to the senses. And my object was solely to explain these obvious facts by means of those assumptions, as effects traced down to their cause, and not in any way to prove them, seeing that they are sufficiently known already. On the contrary, I profess to have proved the latter by means of the former, as causes deduced from their effects.' The very same reasoning reappears in the *Discours* itself.[1] Referring to his *Dioptrics* and *Meteors*, he says: 'The steps in my argument seem to me to depend in such a way upon each other that just as the ones which come last are proved by those which precede them, as effects by their causes, so these earlier ones are proved by those which come later, as causes by their effects. Nor should any one imagine that I have fallen into the error known among logicians by the name of "circle"; for as these effects are for the most part rendered extremely certain by actual experience, the causes from which I deduce them serve less to prove than to explain them: on the contrary, it is the causes which are proved by the effects.'

We see, then, that whether a result is inferred *a priori* or *a posteriori*, it is always deduced after the mathematical model. Deduction, with Descartes, is in fact 'the only form of putting things together which can leave us certain as to the truth of what we synthesise'.[2] There is therefore no room left for any Theory of Induction in the modern sense of the word. Descartes contents himself with remarking that 'whenever we believe that from some particular or contingent fact we can infer a general and necessary conclusion,' we commit a mistake similar to that which we make when we 'conclude that the air is a vacuum on the sole ground that there is positively nothing in this aërial space which we can perceive by sight or touch or any other sense'. In a word, it is not with inferences from particulars to universals, or *vice versa*, that Descartes is primarily busied. His main concern is to keep throughout his reasoning to the guiding line of necessary sequence, and to start always from what is simple and absolute and intuitively

[1] *Discours*, 6ième partie, § 10. [2] Reg. XII., § 90.

discerned. These are to him the sufficient as well as the necessary conditions of all true thinking; whether we choose to call this process by the name of induction or of deduction, of *a posteriori* or of *a priori*, matters little after all.

VII.—*The Rôle of Experience in the Natural Method.*

We have seen that the course of an *a posteriori* proof involved a reference to Experience in the form of Experiment. Experiment had to decide which of several possible causes was the cause sanctioned by Nature herself. Descartes makes a corresponding appeal to Experience in his *a priori* demonstrations. Experience has to decide which of several possible effects has been attached by the Creator to a certain cause. Descartes is therefore constantly reasoning through the help not only of a natural light, but of observation and experiment as well. The part which experience plays in Descartes' system has been the subject of much controversy. The truth of the matter seems to be this, that whilst Descartes is provisionally dependent on experience for most of his particular results, the question as to whether a truth is derived with or without the help of experience affects neither his general results nor, on the whole, the nature of the method by which all these results, general or particular, are reached.[1]

The influence of experience on Descartes may therefore be characterised by saying that it required him to draw at least a provisional distinction between General and Particular Physics. This distinction is, however, purely provisional. Descartes' aim is ultimately to dispense with it altogether by including general and particular results alike under one and the same *a priori* system. His assumptions, as we have seen, are hypothetical only in the sense that he has not yet attached them to his fundamental metaphysical principles by means of an *a priori* chain of argument. He holds his *a priori* reasoning in reserve, at one time because he cannot give it without laying bare his whole system of Physics,[2]

[1] Very occasionally, it is true, a remark of Descartes seems to show us that the facts of experience are compelling him to recognise that his method is not universally applicable, and cannot afford altogether to neglect the nature of the data with which it has to deal. Thus in one of his letters (V.C., x., 203) he asserts that whilst nothing can be satisfactorily included in the science of Physics which is not deduced by a necessity of reason, yet exception must be made for what can be known by experience alone, as that there is only one sun, only one moon revolving round the earth, etc.

[2] *Cf.* V.C., vii., 380.

and at another time because his *a priori* system is not sufficiently developed.[1] But the *a priori* is always waiting its opportunity to encroach on the *a posteriori*, and the general to swallow up the particular. In a letter addressed to 'M. de Cavendish,' Descartes apologises for having 'dared to determine things which depend on experience without having ever brought them to the test of experience';[2] and in another letter we read, 'My *Dioptrics* and *Meteors* where I have deduced from the principles of my Philosophy the explanation of several particulars of everyday occurrence show you after what fashion I am accustomed to reason about natural phenomena'.[3] The distinction in question is therefore of an essentially provisional kind.

Now the *locus classicus* in which Descartes establishes this distinction between general and special physics occurs in the *Discours*.[4] Descartes is explaining to his readers the order he has followed in his systematic pursuit of truth. 'In the first place I tried to discover in a general way the principles or first causes of whatever exists or can exist in the world, and this by considering God only who created it, and consulting nothing beyond certain seeds of truths naturally implanted in our souls. I then examined what were the first and most ordinary effects which could be deduced from these causes; and in this way I seem to have come across the heavens, the stars, the earth, and, on the earth, water, air, fire, minerals, and some other phenomena of the same kind, such as are most common and simple and consequently the easiest to understand. Then when I wished to proceed to those that were more special, such a variety pressed itself upon me that I judged it impossible for the human mind to distinguish the forms or kinds of bodies actually found on the earth from an infinity of other bodies which might have been found there had God so willed it, . . . without passing beyond causes to effects and making use of several special experiments.' In this passage we have an explicit declaration to the effect that in proportion as one penetrates farther into the details of science, experience, in the scientific form of experiment, practically becomes more and more indispensable: particular or experimental Physics must come to the help of the general or mathematical. 'I have been bold enough,' writes Descartes in one of his later letters, 'to wish to

[1] *Discours*, 6ième partie, § 3.
[2] V.C., ix., 521.
[3] *Cf.* also V.C., iv., p. 27.
[4] 6ième partie, § 3.

explain the way in which the animal system develops from the very beginnings of its first existence; I refer here to the animal, taken generally, for as concerns man in particular, I should not dare to undertake the explanation, being short of the experimental evidence such explanation would require.'[1]

With the restrictions noted above, Descartes might well be called an experimental philosopher. His works are full of the evidences of observation and experiment. 'I have proved in detail,' he says, 'about as many facts of experience as there are lines in my writings.'[2] He is constantly being stopped by his inability to pay the cost of certain critical experiments,[3] or through having to wait till they are ripe for testing. 'Let me add,' he writes in a letter to M. Chanut,[4] 'that whilst I am waiting for the plants in my garden to grow up, in order that I may make some experiments on them that will help me to go on with my Physics, I devote some of my spare time to thinking over the special problems of morality.' He himself is an enthusiastic experimenter. During one whole winter at Amsterdam he went almost every day to the slaughter-house to see the beasts killed and to bring home with him such portions of the dead bodies as he desired to dissect at leisure.[5] Thus he tells us in a letter to Mersenne[6] that he is busy dissecting the heads of various animals 'to find out in what imagination, memory, etc., really consist'.[7] He has, further, a most excellent eye for the essential points in an observation or an experiment.[8] Finally, his love of the useful and the concrete constitutes another tie between Descartes and Experimentalism. We have seen that Descartes appeals to experience partly to decide which of several possible causes is the cause sanctioned by Nature herself, and partly to decide which of several possible effects has been attached by the Creator to a certain cause. In both cases Experience serves as the point of transition from the possible to the actual, from the abstractly rational to the concretely real. Descartes is

[1] V.C., x., 121. [2] *Ibid.*, ix., 343.

[3] *Cf. Discours*, 6e partie, *passim*; also V.C., iii., pref., p. 27; ix., 337.

[4] V.C., ix., 413. [5] *Ibid.*, viii., 174. [6] *Ibid.*, vi., 235.

[7] *Cf.* also: V.C., vi., 225; viii., 192, 227; x., 344, 351.

[8] *Cf.* V.C., vi., 97; *cf.* also V.C., vi., 182 and 210, which are especially interesting on account of their allusion to the Baconian method of Induction. Descartes looks on Bacon's method as a very convenient method for others to use when their object is to collect systematically such data as the exercise of his own method requires. *Cf.* Baillet, *Vie de Descartes* (livre ii., ch. xi., p. 149).

intensely alive to the necessity of keeping in touch with reality, and is never weary of inveighing against the misuse of such pure abstractions of the mind as have no counterpart in Nature herself. 'What arithmetician,' he says, 'is satisfied with the bare severance, through an intellectual abstraction, of the numbers he uses from the objects they refer to? What geometer does not go in the teeth of his principles and obscure the transparency of his subject, when he asserts that lines have no breadth and a surface no thickness?'[1]

And yet despite this enthusiasm and this aptitude for experiment, despite this strong bias towards the useful and the concrete, the term 'experimental philosopher' would be a quite misleading one to apply to Descartes. It would be judging Descartes by the means he used to attain his end, rather than by the end itself. This end, as we have already seen, was to give a mathematical explanation of the universe, and it is this *a priori* point of view which, after all has been said on the other side, remains the characteristic and dominant feature of the Method and Philosophy of Descartes.

VIII.—*The Origin of the Natural Method.*

The story of Descartes' Method would be incomplete without some reference to the conditions under which it arose and took shape in Descartes' own mind. The method, as we have seen, is essentially a mathematical method. It is mathematical in its spirit and in the form which it assumes. It is equally mathematical in its origin, as Descartes himself confesses. Moreover, the leading mathe-

[1] Reg. XIV., § 116. As means or helps such pure abstractions may indeed play an important part in the elucidation of phenomena, as is seen in the case of subsidiary angles in Trigonometry, Descartes' own 'indeterminate multipliers,' the circles of the celestial sphere and imaginary quantities generally. 'Do we not in Geometry,' says Descartes, 'make assumptions concerning some quantity or other without weakening in the least the cogency of the proof we give? and yet these assumptions often fail to be borne out by our experience of the real nature of that quantity in Physics' (Reg. XII., § 73). Again, in the case of Mathematics, the very nature of the subject precludes us from asking whether these abstractions have a real foundation in the objects themselves or are the arbitrary inventions of our own mind: 'the question as to whether they have any foundation in reality belongs rather to the domain of Physics'; but once these 'subsidiaries' claim a reality *per se*, they are being misused, and must inevitably lead to error and confusion: mere modes are taken for genuine qualities, and mere numbers clothed with mysterious attributes.

matical methods of his own day are, as we shall try to show, reproduced in the main lines of the universal method which is developed in the Regulae.

In what seems to have been the first public statement of his Method, at a *soirée* in the house of M. de Bagny, given in Paris in November, 1628, Descartes asserted that he knew of no surer means of avoiding error than the observance of a certain method which he had derived from the fundamental principles of mathematics and to which he gave the name of 'Natural Method'.[1] He added that he believed there was no truth which could not be proved by means of it. A precisely similar confession is made in the *Discours*, immediately after the enunciation of the four rules. 'Those long chains of reasoning, so simple and so easy, which geometers are in the habit of using in solving their most difficult problems, had brought me to reflect that all things which men can possibly come to know follow each other in the same connected way, and that provided one never accepts false for true and always keeps to the order required for deducing them one from the other, there are none so distant that they cannot be reached, none so secret that they cannot be discovered.'

This basing of the 'Natural Method' on the principles of mathematics is, in Descartes' eyes, necessitated by the nature of reason itself. Thought has only to be left hidden in its own natural light, and it cannot but conceive methods that are mathematical in their clearness and their certainty. Thus the two forms of analysis which were prominent in the mathematics of Descartes' day are described by Descartes in a significant passage[2] as 'nothing else than the spontaneous fruit sprung from the innate principles of this [natural] method'. Thus the innate principles of Descartes' method are also the innate principles which presided over the formation of these two methods of analysis. In studying these mathematical methods as embodied in the mathematics of his day, Descartes therefore became conscious of the form which his own method must necessarily take if it wished to be natural, or in other words clear and certain.[3] Let us then briefly consider these two methods and see in what way and to what extent they would have been likely to direct the mind of the youthful Descartes towards the discovery of the universal method of the Regulae and *Discours*.

[1] *Cf.* Millet, 'Descartes avant 1637,' *Thèses Français de Doctorat-ès-Lettres*, 2ième série, 1894, p. 152.

[2] Reg. IV., § 19.

[3] *Cf.* Reg. X., § 61.

The first of these methods is what Descartes calls the analysis of the Ancients. We consider the problem as already solved. Then by a process of regressive deduction we simplify it step by step until we reach a fact which is either obviously true or obviously false. If false, the argument has culminated in a *reductio ad absurdum*, and the assumed solution is at once discredited; but if the ultimate fact reached is obviously true, we then proceed to prove the problem synthetically and directly by taking the obvious truth itself as our starting-point and mounting up along a series of successive deductions and by the very steps marked out by the previous analysis until the *demonstrandum* is at length secured. That Descartes was not unmindful of this analysis of the ancients is clear from Reg. V., where we are told that the Method will be observed in every part if we gradually refer propositions that are involved and obscure to simpler propositions, and then, starting afresh from the intuition of all the ultimately simple propositions, attempt to mount up to the knowledge of all the others by means of the very steps that helped us down to our starting-point. The rule might indeed be summarised in the words: 'the whole method consists in following point by point the ancient method of analysis'.

If we turn now to Rules VI. and VII., we shall see in the first place that what the former essentially requires is that at each step in a deduction we should be able to trace the precise nature of the connexion between the quaesitum and the datum. It is in fact 'in this one form of observation that science essentially consists'.[1] We shall also see that the essential requirement of Rule VII. is that all enumerations should be adequate and methodical. Now it is hardly too much to say that these two separate requirements are secured by the use of the second of the two mathematical methods with which Descartes was familiar as a schoolboy, the method of modern Algebra.

It is important to notice that shortly before Descartes appeared on the scene Viète had introduced an innovation in the notation of Algebra which gave quite a new significance to its method. He introduced the use of letters to denote the known quantities of a problem. Before Viète these known quantities had always been expressed arithmetically, and the consequence was that by means of addition, subtraction, multiplication and division the numerical quantities representing the data so ran into each other that it soon be-

[1] Regula XVI., § 131.

came quite impossible to trace the relations between quaesita and data. Viète's reform enabled one to see at a glance at any step in the solution what these relations were. Thus $(a+b)$, $(a-b)$, ab, $\frac{a}{b}$, $\sqrt{a^2+b^2}$ are expressions which tell their own tale; to look at them is to see at once the operations made to obtain them. But if we write 7, 1, 12, $\frac{4}{3}$, 5, what is there to tell us that 7 is obtained by adding 3 and 4 together, that $1=4-3$, that $12=4\times 3$, that $5=\sqrt{4^2+3^2}$?—12, for instance, might just as well have expressed the product of the factors 6 and 2, and 5 might have expressed the result obtained by adding 2 and 3 together.

That Descartes was fully alive to the importance of this characteristic feature in the notation and method of modern algebra is obvious from Reg. XVI., § 129.[1] 'We are here making abstraction,' he writes, 'of numbers, no less than we did a little further back of geometrical figures, and of every other thing. Our reason for so doing is . . . chiefly that those portions of the matter which contain the essence of the difficulty shall always remain distinct, and not get mixed up with numbers that tell us nothing at all.' It is by keeping the elements in a solution separate throughout the process that Descartes is able to compare the different steps and results with each other, and note which of them is simplest and wherein the complexity of the others consists.

The new algebraic notation points thus in precisely the same direction as Reg. VI. The same may be said of it in reference to Reg. VII. Thus let $axx+bx+c=0$ be an equation written down *à la* Viète. Solving it we find that $x=\frac{-b\pm\sqrt{bb-4ac}}{2a}$. Hence we conclude by means of an adequate and methodical enumeration that if $bb\succ 4ac$, the equation has two real roots; if $bb=4ac$, the roots are equal; and if $bb\prec 4ac$, the roots are imaginary. But if the equation is given in the form $2xx-11x+12=0$, we find on solving it that $x=\frac{3}{2}$ or 4, and enumerations are neither possible nor called for.

These comparisons may seem at first sight to be somewhat superficial. But we must remember that from a very tender age Descartes had busied himself with algebraical equations, and that the two issues which a mathematician has perpetually before him in the theory of equations are

[1] *Cf.* also a page of Descartes' correspondence, ed. Victor Cousin, ix., 150.

just these two: (1) the form of the connexion between data and quaesita; (2) a complete enumeration of all possible cases. Let us take a typical example.

Let α, β be the roots of the equation $ax^2 + bx + c = 0$ . . . (1). Then we have, in virtue of Harriot's discovery with which Descartes was quite familiar,

$$a\,(x - \alpha)\,(x - \beta) = 0$$

$$i.e.\ a\,(x^2 - \overline{\alpha + \beta}\,x + \alpha\beta) = 0 \quad . \ . \ . \quad (2).$$

The question now is: what are the relations connecting α, β, the two values of the unknown quantity x, and the quantities of which we are in search, with the given quantities a, b, c? Comparing the two identical equations (1) and (2), we get at once by equating coefficients,

$$\alpha + \beta = -\frac{b}{a};\ \alpha\beta = \frac{c}{a},$$

results of great generality and of fundamental importance. By means of these two equations we deduce the values of α, β.

Thus $\alpha = \dfrac{-b + \sqrt{bb - 4ac}}{a}$; $\qquad \beta = \dfrac{-b - \sqrt{bb - 4ac}}{a}$.

The question now is: what are the relations which must hold among the data a, b, c, in order that these roots may be respectively real, imaginary, or equal? A methodical and adequate enumeration of all the possible cases shows us at once that the roots are real, imaginary or equal according as bb is greater than, less than, or equal to $4ac$.

This example may help us to realise the extreme importance of Viète's notation, and to feel the influence which the new method this notation first made possible was likely to exercise over the mind of the young philosopher, who at the first was a mathematician rather than a philosopher, and who, in his search after the true method, deliberately allowed himself to be guided by the indications of Plato's analysis and the modern Algebra of Viète.[1]

We have now seen what a large share these two recognised mathematical methods of Descartes' own day must have had in the suggesting and shaping of his own natural method. They anticipate, as we have seen, the main content of the Rules V., VI. and VII. Now these rules 'state and unfold the precepts of methodical order';[2] they are, in Descartes' opinion, 'the only ones which the pure reason need observe in the preliminary handling of any problem'. In other words, they contain the whole method of Descartes.

[1] See *Discours*, the paragraph immediately following the enunciation of the four rules.

[2] Reg. VIII., § 44.

IX.—*The Originality of Descartes.*

In dealing with the question of the origin of Descartes' method, we have been led to see that it grew naturally out of Descartes' own mathematical studies. In this sense it was no original creation, being in its essence as old as mathematics and natural reason itself. Descartes' originality is the more powerful on account of there being nothing miraculous or unaccountable about it. His discoveries do not flash from him like Minerva from the head of Jupiter, but are bound up with the intense unity, or as we might put it, concatenation of his rational life. 'I am much obliged to you,' he says in a letter to a friend, 'for professing delight at my not having allowed myself to be anticipated in the publication of my ideas. I have never had any fear, however, on that score; for, apart from my indifference as to whether I may chance to be the first or the last to write what I do write, provided only that what I write is true, my opinions are so welded together and depend so vitally on one another, that no one of them could be appropriated unless all the others were previously known.'[1]

It is the context in fact which can alone explain the text, and, in the case of Descartes, the true context of every truth he discovered was the whole system of knowledge which his method had mastered and organised.[2] In this systematised knowledge lay the secret of Descartes' strength and the source of his originality. In his opinion a man is original, not through happening to be the first to discover or invent this or that, but in so far as he has made the thoughts and truths he deals with his own, incorporated them, as it were, into the tissue of his own mind. 'Whatever you know is yours entirely,' he says in a letter to a friend, 'even though you may have learnt it from some one else.'[3]

It is this assimilation of a truth as an integral part of one's rational life which stamps its utterance as original, and makes plagiarism quasi-impossible. 'Water,' we read,[4] 'always looks like water, but its taste when drawn from a spring is one thing, when drawn from a pitcher, quite another.' Descartes is therefore very little concerned about the novelty of his opinions. 'I cannot sympathise with those,' he says, 'who desire that their opinions should appear novel; on the contrary, I try to suit mine to those of others so far as truth permits me.'[5] He disclaims, in fact, any pre-

[1] V.C., vii., 380. [2] *Cf.* V.C., vi., 53, 105, 243; vii., 5, 380.
[3] *Cf.* V.C., vi., 148; also viii., 442. [4] *Ibid.*, vi., 150.
[5] *Ibid.*, ix., 166.

tence of originality in the sense of novelty or strangeness: he is anxious not so much to know first as to know truly. 'As for opinions which are entirely my own,' he says, 'I make no apology for their novelty inasmuch as I am certain that one has only to consider well the reasons which I give for them to see that they are so simple and so appeal to one's common sense that they must seem less extraordinary and out of the way than any other opinions one can entertain on the same subjects. Nor do I boast in the least of being the first to invent any of them; my cause of boasting is rather this, that I have never accepted an opinion simply because others have or have not entertained it, but solely because my reason has persuaded me to do so.'[1] And this is the Alpha and the Omega of the method of Descartes.

[1] *Discours*, 6ième partie, § 10.

IV.—A CONTRIBUTION TOWARDS AN IMPROVEMENT IN PSYCHOLOGICAL METHOD. (III.)

(*Conclusion.*)

By W. McDougall.

Remarks on the Nature of Neurosis in General.

Hitherto, I have dealt with only the simple motor reactions of the nervous system, and before attempting to deal with more complex functions of the mind, it is necessary to say something of the probable nature of neurosis in general. So far as we know, the nervous system consists wholly of distinct units, the neurons; *i.e.*, the nerve cells and their processes. The neurons are in groups and sub-groups, the members of which are in functional continuity of varying intimacy, through proximity of the terminal twigs of the processes of one with similar twigs or with the cell bodies of others. It is commonly agreed that a simple reflex action consists in the passage of nervous impulses through a group of such neurons arranged in a more or less serial manner, leading from the afferent to the efferent nerve fibres. It is a not improbable assumption that this is the type of all neurosis; that the neurosis concerned, in the most complex mental activity, consists also of the passage of an extremely complex series of nervous impulses, through extremely complex groups of neurons from the afferent towards the efferent side; the impulses being variously reinforced in their passage through the cell bodies. According to this view all mental process is but a complication of motor re-action to stimuli affecting the sense organs; the neurosis concerned in cognition and reasoning is essentially conative in character, all the neural impulses pass onwards towards their natural goal, the nuclei of the efferent nerves. In man, a very great complication may be assumed to have resulted from the acquirement of speech: a very large part of the nervous impulses find their goal in the motor speech-centres without actually determining movements of the organs of speech. It is when the nervous impulses find their natural goal in appropriate motor innervation that they rapidly become organised on repeti-

tion, and lose their conscious accompaniments. This is well illustrated by our first example of the morning call. In this case, there are two distinct motor outlets for the impulses excited by the sounds, namely, the innervation of the speech organs, and that of the muscles that raise the body. As the response becomes automatic, the energy of the nervous excitement becomes drawn off more and more into the tracts leading to the speech organs, for on each repetition of the answer the afferent impulses from the muscles used will tend to organise still further the tracts concerned; and so the nervous excitement, finding a natural outlet in the movements of the speech organs, soon ceases to pass into the tracts leading to the getting-up muscles. It is, I think, a fair assumption in the case of our last example, that if the waiter were constitutionally lazy and in the habit of answering calls with a simple 'Yessir,' and no other immediate movement, then he might answer 'Yessir' while continuing to sleep.

I have already made use of this assumption as to the general nature of neurosis, and I shall continue to do so in the following pages for the sake of convenience of description, since it seems to be well founded and highly probable.

If we adopt this view of neurosis, we must suppose that when neurosis is accompanied by psychosis, consciousness is determined by the passage of the nervous impulses along the whole or some parts of the tracts of neurons concerned, in whatever part of the nervous system they may lie. For I have already pointed out that the assumption that consciousness is generated in some particular part of the brain, whether in the pineal gland, the medulla oblongata, or the frontal lobes, is quite unfounded and unnecessary. Here I would point out that the evidence is all the other way. For all that we know of localisation of function in the cerebral cortex, and especially of those cases in which by a local lesion, such as a hæmorrhage or an embolus, the possibility of some particular form of consciousness is abolished, and of the phenomena of the epileptic aura, indicates that consciousness is immediately determined by neural processes, now of one, now of another part of the cortex. The phenomena of double personalities and of split-off parts of the self forming minor personalities indicate the same fact. For these cases are only explicable by the assumption that groups of neurons become functionally discontinuous with the rest of the nervous system, and form a separate minor nervous system capable of producing consciousness, and even some degree of self-consciousness.

The Conditions of Occurrence of Consciousness in Terms of Neural Process.

These considerations prepare the way for the attempt to describe complex processes of the mind in terms of neural processes, and to define more accurately than has yet been done the conditions of occurrence of consciousness in terms of those processes.

In making this attempt, I cannot do better than to begin by accepting without reserve the doctrine of noetic synthesis and apperception as laid down by Stout in the second volume of his *Analytic Psychology*. We read there that 'In the developed consciousness, the whole mind is more or less perfectly organised into a system of noetic units, in which partial apprehensions are subordinated to more comprehensive apprehensions, and these in their turn to others still more comprehensive'. The noetic units or syntheses are mental dispositions built up by apperception, a process by which every presentation is appropriated by some noetic unit. 'Apperception may be defined as the process by which a mental system appropriates a new element, or otherwise receives a fresh determination;' it is the process by which 'products of past process determine and are determined by succeeding changes'.

As we have already seen, Stout avoids physiological considerations by adopting the hypothesis of psychical dispositions. The latter seems to be the same thing as a mental system in a passive state, and Stout explicitly states that it may be regarded as a physiological fact. If we attempt to express this doctrine of mental systems and apperception in terms of neural process, we shall find that there is implied in it the proposition whose truth I am endeavouring to establish, namely, that consciousness accompanies only the process of establishment of new connexions among neurons. For a mental system in its passive state is in its physiological aspect a grouping of neurons forming a more or less complex path, by which afferent nervous impulses pass on towards their efferent goal in a manner determined by the nature of the path. The activity of a mental system consists in the passage of nervous impulses through it in this manner. The mental system has been built up from a much simpler nucleus, probably a simple reflex nervous arc by repeated apperception, 'the process by which a mental system appropriates a new element, or otherwise receives a fresh determination'. In terms of neural process, apperception must be conceived as an extension and further complication of a mental system, by the incorporation into it of other neurons and systems of

neurons, so that the complex path no longer leads to quite the same, but to a modified efferent outflow. This process seems to be determined in two ways. It may be by the presence of some novel element in the afferent group of impulses, for whose reception and transmission the system is adapted, so that the system is no longer perfectly adapted for their transmission; impulses then overflow the physiological barriers of the system, spreading over to neurons hitherto unconnected with the system; the physiological barriers once passed are destroyed or partially destroyed, there is established a connexion between the system and the neurons newly affected, the latter are incorporated into, and henceforward form a part of the former,—the mental system has appropriated a new element. The other way in which a system may receive a fresh determination is by the establishment of inter-connexions with other systems, through their excitement contemporaneously, or in close proximity in time, *i.e.*, by association by contiguity in time. For there are good reasons for believing that the physiological barriers of a system are not absolute; that when a system A is excited, although the main current of the flow of impulses passes through the nerve-paths constituting the system, yet a fainter excitement spreads from the system through a large area of the whole nervous system. Further, we must believe that if any other system B, forming a part of this faintly excited area, be excited at the same time or immediately afterwards, then from it also there spreads a faint excitement through a large area and passes with especial ease and force along the tract by which the faint excitement has spread from A to B. This tract is then more permeable in the future, and any excitement of A tends to spread to B also, and conversely. Between the two systems A and B there is thus established a connexion, and they form now a single, more complex system, of which A and B are sub-systems. The motor re-action to which the excitement of this new system leads, is the resultant of the re-actions of the two systems, and may appear as a more complex re-action, showing the features of both the constituents, or it may appear as a partial abolition or inhibition of the one by the admixture of the other, according as they are similar or antagonistic in nature.

Whether apperception is ever determined in the former manner, without some previous effect of the latter kind, or whether in fact the two processes are not essentially similar in nature, is an interesting but difficult problem, the consideration of which is not essential to the purpose in hand.

By relatively simple mental systems, our ordinary automatic movements are carried out. The paths have been organised by frequent repetitions of similar series of afferent impulses, each of which, in so far as it was different from its predecessors, determined apperception (the extension and complication of the system by the establishment of new connexions), and so modified the motor re-action; while the part of each series of impulses that was coincident with its predecessors ran in the paths already organised. In purely automatic movement, simple or complex, the series of afferent impulses are all coincident with previously experienced series, they run to their motor goal through well-organised systems, there is no making of new nervous connexions, no apperception, no consciousness. In learning any complex action, there is at first much varied attention, first to one part, then to another, and to the impressions from them; there is much consciousness, continual apperception, *i.e.*, continual organisation. Then as parts of the total complex movement become automatic, while others still require attention, the movement is in part carried out by organised paths or sub-systems, without further apperception and without consciousness, and in part by sub-systems that are still undergoing fresh determinations, still involving apperception and consciousness. In every volitional movement, as distinguished from purely automatic movement, there are concerned neural processes of these two kinds; a large part of them are automatic, proceeding to their motor goal by organised sub-systems, while some pass partly by new or relatively new paths, and in so doing organise them, *i.e.*, there is apperception, attention and consciousness.

Turning now to cognition, and keeping its conative aspect before us, we must recognise two kinds of process concerned in any apprehension of an object. The part of the object presented in consciousness is but a part of an implicitly apprehended whole. The implicit apprehension consists in the excitement of a mental system corresponding to the previously presented parts of the object; the impulses excited by these parts pass to their appropriate motor goal by well-organised paths without exciting consciousness. But any novel elements in the object excite impulses that find no such organised paths, they determine an extension of the system by incorporation of new elements; there is establishment of new connexions, *i.e.*, apperception, and with this establishment of new connexions consciousness is aroused, a consciousness of all the novel elements that are thus apperceived.

Thus, to make use once more of the much-used orange, when I see an orange and go to take it up, I am conscious of its general shape and colour, and chiefly of its position, but there is much besides that is implicitly apprehended. If I take it in my hand, the strength of the muscular contractions, involved in raising it, is accurately pre-adapted to its weight, as shown by the fact that if it is heavier or lighter than oranges usually are, I fail to move it in the first moment, or it flies up unnecessarily high. The weight of the orange was implicitly apprehended, but I was not conscious of it. It is not, as Spencer seems to imply in discussing this example, that I consciously expected in a dim way a certain weight. It is merely that a mental system corresponding to oranges in general has been built up in the brain, and when I put out my hand to take it up, nervous impulses are appropriately co-ordinated in strength and direction by a sub-system which has been built up by previous apperceptions of the weight of oranges; the excitement of this sub-system constitutes the implicit apprehension of the weight of the orange.

I do not, for the purpose in hand, make any distinction between the afferent nervous impulses that lead to the perception of objects in the external world, and those from the muscles, joints and other structures that are concerned in the carrying out of movements. The afferent impulses concerned in the carrying out of an automatic movement, or those parts of a complex volitional movement that go on automatically, are implicitly apprehended in exactly the same sense as those originated by the parts of a complex external object that though not attended to play a part as the 'fringe of thought' in determining the whole mental process. It is only when series of afferent impulses are frequently repeated with but very slight differences, and without fresh determination of the mental system from within, that the apprehension of them becomes wholly implicit, that the mental system excited becomes completely adapted to transmit them to their appropriate motor goal, without any fresh apperception, without any consciousness. These conditions are seldom realised except in the case of rhythmically repeated movements, such as those of walking and cycling. When we look at a complex object there is continual apperception, for such an object contains many parts in many different relations to one another, and it can scarcely be presented twice without change in the relations of its parts, either to one another, or to its surroundings as they affect the sense organs, or without some change having taken place in the mental system concerned; hence as Stout says: 'In almost every moment of waking

life, an apperceptive process is taking place. Whenever an object is attended to, the presentation of it is apperceived.' It is instructive in this connexion, to try to attend continuously to a minute black spot without distinguishable parts on a smooth white surface. In this object there is little or nothing to be apperceived. We can only remain intermittently conscious of it by apperception of its relations to any other distinguishable objects in the field of vision or by continually bringing it into relation with some presentation of self, a process in which language seems to play a large part. To remain conscious of the simple point itself would be inconsistent with the very nature of thought; for thought is a process of continual transition, to arrest its flow but for a moment is to destroy it.

In every complex object that affects the sense organs, we must distinguish three kinds of parts. Firstly, there are parts that are undiscriminated because there is no mental system corresponding to them; they fail to affect mental process in any way; they form the undifferentiated part of the objective continuum, a part that may be said to be in the preconscious stage as regards the particular mind that they fail to affect. Secondly, there are those parts which are apperceived, to which we attend, of which we are conscious. Thirdly, there are those parts that, though not attended to, are implicitly apprehended; they may be said to form the post-conscious part of the presentation-continuum.

Apperception then is one aspect of the attention process, and consciousness occurs only in association with apperception, *i.e.*, it occurs only as the immediate correlate of the process of establishment and organisation of new connexions between neurons and systems of neurons.

I have said that this conclusion is implied by the doctrine of apperception and noetic synthesis, as put forward by Stout, and I will here make a somewhat lengthy series of quotations to justify this statement, and so lend weight to my conclusion. In vol. ii., p. 3, we read, 'The antithesis between noetic synthesis and contiguous adhesion is brought out in the clearest light in the passage of attentive into automatic process. In proportion as automatism supervenes, association becomes substituted for thought-control, and in the final stage completely supplants it;' on p. 9, 'It is not enough that the residua of previous impressions should be re-excited. Their re-excitement must bring with it a peculiar modification, resulting in a new growth both mental and physiological;' and on p. 40, 'When we consider a noetic synthesis not merely as involved in this or that conscious process, but

as a mode of mental grouping which persists as a disposition when it has ceased to operate in actual consciousness, we have the idea of an apperceptive system. It is through the gradual growth and differentiation of such systems that new phases of noetic synthesis arise.' On p. 73, in treating of relatively automatic movements he writes, 'They are automatic parts of a process which as a whole involves concentrated attention. Attention is engrossed by a series of fluctuating conditions and by the nature of the end to be attained. But the adaptation of movements to the perceived conditions takes place in a more or less automatic way;' and on p. 87, in treating of conation, 'It is separately felt as such, when and so far as the special conditions of its partial revival vary in a way which is not thoroughly familiar, and so call for a re-adaptation by relative suggestion or otherwise, which cannot take place in an automatic manner'; on p. 112, 'The process of apperception substantially coincides with that of attention. . . . The attention-process, as we saw, is characterised by systematic unity and relative novelty. Under the head apperception, we are especially concerned with the relations of the new to the old, in so far as it gives rise to modification of the old. We consider the whole process as one by which the mind grows;' on p. 113, 'The need for the apperceptive process and the stimulus to it lies in the initially foreign and alien nature of the new element, and the consequent antithesis between its disturbing novelty and the internal self-development of the system'; on p. 115, 'The whole itself is nearly always presented in the way of implicit or schematic apprehension. To say nothing of other difficulties, the narrowness of consciousness forbids that it should, at any moment, be presented in the fulness of its details. Hence a mental system, even when it enters into conscious process, always remains to a very large extent beneath the threshold of consciousness;' on p. 118, 'Where attention is not present, there is no apperception, but mere assimilation. . . . Assimilation there must always be, inasmuch as the existence of a given experience coincides with the re-excitement of some pre-formed disposition. Thus in automatic actions, the impressions which guide us are all assimilated, but not apperceived. Whenever, through habitual exercise, an organised group of psychical dispositions has become so pre-conformed to a special class of familiar experiences, that it assimilates them with a certain degree of ease and rapidity, apperception becomes needless. There is the less need for it, the more perfect is the mental pre-adjustment.' And on p. 122, Stout himself deals in physiolo-

gical process, instead of psychical dispositions, for he writes, 'Even when the controlling guidance of a central thought ceases to operate as a unifying principle it may leave behind it, as a substitute, a nervous arrangement, which fulfils an analogous function independently of consciousness'.

Completion of the Analysis of a Simple Case of Experience.

It is interesting to return now to the analysis of the spoon-game, the instance that was chosen as a simple case of experience and the type of all mental growth, and to complete it by the application of the doctrine of apperception. We saw that the total result of the game on my mind or nervous system was to establish an automatic re-action to a certain feature of the objective continuum hitherto undifferentiated, namely, the peculiar relation between a stimulus A, constituted by a particular voice speaking my name into my left ear, and the particular stimulus B, the heat of the spoon on my cheek. On the first occurrence of A, it occasioned, in virtue of its character merely as a loud sound in my left ear, the excitement of a mental system long ago built up by apperceptions of many loud sounds, and the excitement of this system leads automatically to the turning of my head to the left; the stimulus in its character as a loud sound is in the post-conscious stage, the re-action is reflex or instinctive. But the stimulus has other features; it is apperceived by mental systems in higher levels of the brain, corresponding to the sound of my name, as my name spoken by a particular voice; the excitement of this system, with the accompanying apperception and consciousness, leads or tends to lead to some such motor re-action as the saying 'Yes? Well! what is it?' But as yet there is no mental system corresponding to the peculiar relation between A and B. When B occurs there is in a similar way a post-conscious re-action, in the jerking away of my head, to B in its character merely as a violent stimulus to the side of my head; while at the same time B in its character as a hot object on a certain spot of my cheek, is apperceived by a system higher in the brain; I am conscious of B as such. Then owing to the contiguity in time of the excitement of these two systems, appercipient of A and B respectively, there is established a connexion between the two, each is modified by the other, and this mutual modification is the apperception of the causal relation between A and B, and there is differentiated a new sub-system corresponding to this relation. This rela-

tion now enters for me upon its conscious stage of existence. On the recurrence of A the new sub-system apperceives it as A-related-to-B, and leads to a new motor re-action, consisting in a modification of the one re-action by the other, a resultant of the two. When this new object A-related-to-B has been presented several times, with but very slight variations in its constituent parts, the elements of novelty in it disappear, consciousness diminishes, and soon the sub-system leading to the new motor re-action becomes completely organised, the relation of A to B enters for me upon its post-conscious stage, and the whole process goes on automatically, while attention is directed elsewhere. There seems to be another principle at work also, one that was noted in connexion with the analysis of the morning call and the automatic response, the principle, namely, of the condensation of nerve paths, the tendency for all the energy of a stimulus to be drawn off into one of two alternative motor centres. For A ceases to be either apperceived or implicitly apprehended, as my name spoken by a particular voice; the corresponding mental system that leads to the response 'Yes? What is it?' does not become organised, but simply ceases to be excited.

The Premisses of the Preceding Argument Generally Admitted.

I have already attempted to show that my thesis as to the occurrence of consciousness is found to be contained in Stout's doctrine of apperception, as soon as we translate it into terms of neural process, and I wish to point out that the main premisses of the argument are very generally accepted. What I have said of the more definite organisation of the paths of the lower parts of the nervous system, and of the greater complexity and possibility of establishment of new connexions in the higher parts, is well put in the following sentences by Waller:[1] 'The higher the brain the greater the mass of the formed formable material. But the further the differentiation shall proceed of formable material, the further it must already have proceeded of formed material. The higher brain must include subordinate highly specialised and economically working mechanisms—guiding parts.' 'And yet the further the brain has developed within the span of life of its possessor, the greater the differentiation of function and of organ out of the mass of formable material.' 'The

[1] *Brain*, xv.

higher the brain the less the fixity, the older the brain the greater the fixity.' And again, 'At the lowest level the representation is most definite and fixed, the canalisation of conductors is most closed, the differentiation of the central organ is most complete. At the highest level the representation is least definite and most fluctuating, the canalisation of conductors is most open, the differentiation of the central organ is least complete.'

James gives expression to the same truth when, after defining instincts as automatic re-actions of the nervous system, he writes that man differs from animals in having not fewer but more instincts, 'though the animal richest in reason might be also the animal richest in instinctive impulses too, he would never seem the fatal automaton which a merely instinctive animal would be' (vol. ii., p. 393).

The fact, that consciousness accompanies novel and relatively novel affections or activities of the brain and dies away when these activities are repeated, is also fully recognised. Thus Wundt: 'Jede Uebung besteht in der Mechanisirung ursprünglich mit Bewusstsein geübter Willenshandlungen'.[1] So also Bain in a recent article in MIND (vol. iii.). 'They (the intellectual trains) also become conscious according as Habit has not supervened to give them a mechanical or automatic flow'; and again: 'Actions properly voluntary lose their character under two extremes or gradations—on the one hand, their shading into the Reflex, and on the other hand, their passing into the Habitual. In both cases, they part to a corresponding degree with their conscious character, as is seen by their giving room for other occupants of the conscious area.' Without professing to understand in what sense the Reflex and the Habitual are two extremes, I regard these sentences as containing a very frank recognition of the unconscious mental constituent on the part of a leading exponent of pure psychology.

But although these two great groups of facts are thus fully recognised, their significance when considered together and in conjunction with the general character of neurosis, has, so far as I know, never been fully and explicitly pointed out as giving the clue to the conditions of occurrence of consciousness. In face of them and of all the considerations adduced above, it must, I think, be admitted that the essential condition of the occurrence of consciousness is the making of new nerve paths, the establishment of new functional connexions between neurons.

[1] *Philosophische Studien*, p. 378.

Some Corollaries of the above Proposition.

It will be seen that, accepting the principle of continuity, it is a logical inference from this doctrine that all adaptation of nervous re-action to environment in the past has been accompanied by consciousness; that the organisation of the simple reflexes and instincts of the lower animals was accompanied by a consciousness of a very low kind; that in man the reflexes of the cord and lower parts of the brain were organised long ago, before man was man, and that some low form of consciousness accompanied their organisation, just as in the adult there are automatic habitual actions carried out by nerve paths that were organised during childhood with an accompanying consciousness.

It is a further inference that, under two conditions, consciousness would become impossible, either if an animal should become perfectly adapted to its environment, or if an individual should have lived so long that all the parts of its nervous system had become mapped out into well-organised paths of automatic reaction. For in the case of an animal perfectly adapted to a limited environment, all its movements would be automatic, all apprehension would be implicit, all its mental processes would go on without consciousness. There would be no apperception, no establishment of new relations among nerve paths in response to novelties in the environment, if there were no such novelties, if for every possible combination of stimuli there had been organised a mental system leading to an appropriate motor re-action. It seems probable that such a state has been almost reached by such lowly animals as Lingula, a Brachiopod, that seems to have persisted as a species unchanged from Pre-Cambrian times up to the present day. Living in the deep sea, such an animal is exposed to an environment of very limited complexity. It may be reasonably supposed that its nervous system is completely organised to carry out automatically its few motor re-actions to the comparatively simple impressions made upon its sensory nerves.

But there is little reason to hope or fear that we shall attain 'Nirvana' by this route. Our environment seems to be infinitely complex and varied; and in the past, natural selection, or whatever forces have determined evolution, seems to have so worked that while adaptation has proceeded by establishment and organisation of new nervous connexions, there has been at the same time a continual increase in the number and complexity of new neurons (those of level 4 in our scheme), affording possibilities of the forma-

tion of new and more complex combinations of nerve paths, of more accurate, detailed adaptation to an increasingly complex environment. And it would seem probable that this multiplication of neurons has been, and will continue to be effected at a relatively more rapid rate than their organisation into fixed paths takes place, so that there are ever increasing possibilities of the establishment of new relations among nerve paths, and of a more intense and more varied consciousness. This must be the case in progressive species at least. The highest animal is not the one whose nervous system is most completely organised for re-action upon a limited environment, and in which consciousness no longer occurs, but the one whose nervous system affords the greatest possibilities of new adaptations, of new relations among nerve paths, and so of the most complex and intense consciousness.

On Painful Consciousness and the Action of Anæsthetic Drugs.

There is one form of consciousness, and one only, which, as it seems to me, it may be proper to describe as 'Anoetic,' and that is the consciousness of pain in certain cases. Pleasurable feeling may be said to occur always in conjunction with healthy normal mental process; there is always some noetic element in the consciousness. Perhaps in painful feeling too, there is always some noetic element, some localisation or recognition of the pain as such or such. But certainly the pain itself may be very intense, while all other contents of consciousness are of very low intensity. By the one word pain, we denote both the sense of strain or of unpleasant tone accompanying the higher mental processes, and physical pain arising from violent stimulation of afferent nerves, or stimulation of nerves in certain abnormal states. Although these two things in extreme cases seem very different, yet they are commonly recognised as being essentially similar. The comprehensive formula for pain, given by Stout, seems to apply strictly to the former kind only. The formula runs: 'An activity which is thwarted or retarded, either by the presence of positive obstructions, or by the absence of co-operative conditions, or in any other conceivable way, is painful in proportion to its intensity and complexity'. And again: 'The antithesis between pleasure and pain is coincident with the antithesis between free and impeded progress towards an end'. We may apply this formula to the analysis of a state of acute vexation. When I am in this distinctly painful state I have a strong feeling of the

necessity for doing something, and yet nothing can be done. There is no appropriate outlet in motor innervation for the surplus of nervous energy excited; I make various violent purposeless movements and explosive utterances; I form rapidly, and as rapidly give up, various projects for immediate action, perhaps all impossible, and utterly inconsistent with one another. This may perhaps go on until I conceive the idea of something to be done to remove the source of vexation. At once the vexation disappears, as I set about giving effect to my idea, the nervous excitement finds vent in rapid and effective trains of thought and action. If we attempt to express all this in terms of neural process, we must recognise that the essence of the whole matter is the setting free of excessively powerful nervous discharges that do not run a normal course to an efferent goal. Instead of flowing through co-ordinated mental systems in an orderly manner to an appropriate goal, and setting up new relations between allied systems and sub-systems, the energy of the discharge is so great that besides flowing in natural paths, it breaks across the physiological barriers that demarcate the systems from one another in an utterly disorderly and unmeaning manner, having little or no reference to the paths determined by heredity and experience; hence the utterly disorderly, though intense consciousness, hence the incongruous ideas, the ineffective explosive movements, the general visceral disturbance of heart, respiration and blood-vessels. While there is a noetic element in the aroused consciousness, the result of apperceptive processes among the mental systems, of the passage of the nervous excitement across certain physiological barriers in an orderly normal manner, there is also the intense unmeaning element due to a similar process of passage across physiological barriers, but on a greater scale, and in a disorderly manner, and this is the painful or displeasing element.

In the same way, physical pain may be regarded as an intense disorderly consciousness accompanying the spread of powerful nervous impulses through large areas of the nervous system. The impulses are so violent that they break across the barriers that suffice to demarcate systems for currents of normal strength, and in so doing they arouse violent unco-ordinated motor re-actions, both visceral and somatic.

There may perhaps be distinguished two determinants of this disorderly form of consciousness: on the one hand, a disorderly, on the other, a too intense initial disturbance. In the case of painful mental activity we recognise the former

determinant of the painful element, in the excitement of systems that cannot work harmoniously together towards the production of a co-ordinated efferent outflow, the presence of conflicting ideas and tendencies; and in the case of physical pain, we recognise it in the excitation of sensory end organs by unwonted stimuli, stimuli that lead to an unwonted set of afferent impulses, for whose reception there is no mental system prepared. Thus, any electric stimulation of the skin, being altogether unusual and unlike any other kind of excitation, is painful or disagreeable, even in its lowest grade of intensity. The other kind of determinant of pain, the too great intensity of the initial disturbance, is also capable of causing pain of both kinds. In the case of pain of mind (as it is commonly called) we have good evidence of this in the way that even pleasurable emotion may become painful through too great intensity. And physical pain seems to result from too violent stimulation of perhaps any afferent nerve. In both cases we may suppose that the nervous excitement is too great to be carried off by any co-ordinated system of nerve paths, that it breaks across physiological barriers on every hand spreading through unrelated mental systems in a disorderly manner, perhaps through every part of the nervous system, so provoking the characteristic display of violent purposeless motor re-actions, both visceral and somatic, and, when very intense, leading to the state of complete nervous exhaustion that we know as 'collapse'. It is significant that severe pain that persists usually sets up some rhythmically repeated motor re-action, such as the swaying of the body, the grinding of the teeth, the wringing of the hands, or the repeated utterance of some irrelevant phrase. The setting up of such a rhythmic re-action leads to a diminution or cessation of the pain; a tract is established and partially organised, and by it the energy of the nervous excitement is partially drawn off, so that the breaking through of physiological barriers no longer occurs on so great a scale.

If this be a true account of the nature of painful neural process, do we not see here a physiological basis for the spiritual benefits of pain, that pious elders so delight to dwell upon? The violent nervous impulses break across barriers that might never have been surmounted otherwise, and so make the first step towards the setting up of relations between mental systems that might have remained for ever unrelated if the individual had lived a happy, painless existence. 'Happiness is dulness,' as George Meredith has somewhere written.

These remarks on the nature of the physiological correlate of pain lead naturally to the consideration of the nature of the action of anæsthetic drugs, such as alcohol, chloroform and ether. I will set down here an hypothesis as to the nature of their action which, while it seems highly probable, would, if it could be shown to be true, lend powerful support to my main thesis as to the physiological conditions of the occurrence of consciousness. All the facts known as to the action of these drugs seem to me to point to the conclusion that they tend to diminish and abolish functional continuity between the neurons, just as curari abolishes functional continuity between neuron and muscle fibre, and atropine that between neuron and gland cell.

In the first place, we know that the activity of the central nervous system is profoundly affected by the presence in the blood of such small proportions of these drugs as do not appreciably affect the conductivity of nerve fibres. And we know that the higher mental functions are abolished while the neurons, constituting the reflex arcs of the cord, are not appreciably affected. There are no grounds for believing that the chemical natures of the cells concerned in the higher and lower functions of the nervous system are essentially different.

But far more important is the fact that the functions of the parts of the nervous system seem to be abolished strictly in the inverse order of the degree of fixity of organisation of the nerve paths that subserve the functions. For, beginning at the bottom of the scale of functions, we know that the last functions to be abolished by these drugs are those of the respiratory reflex arcs, and it may fairly be assumed that those through which nervous impulses are passing in every moment of life, from birth onwards, are the most completely organised of all reflex paths. Earlier than these are abolished such pure reflexes as the corneal and those on which the tone of the skeletal muscles depends, and earlier still, such reflexes as lead to the quick withdrawal of a limb on sharp stimulation, and all such reflexes as are only of occasional occurrence, and approximate in type to instinctive actions. Pharmacologists describe the effects of these drugs on the nervous system in general terms, as the abolition of its functions in the order from above downwards, in a scale not very accurately defined. If we analyse the early stages of the effects of alcohol, we find that, just as in the case of the reflexes of the cord, the functions of the nerve paths of the cerebrum are abolished in the inverse order of their degree of fixity of organisation, the degree of intimacy of physiological continuity

between the constituent neurons. Recent experimental work by Kraepelin has shown that alcohol, even in doses so small as to produce no subjective symptoms, diminishes intellectual power and accuracy. The first well-marked symptom is a diminution of self-consciousness. It is notorious that a man when slightly under the influence will boldly do and say things that in his normal state he could not bring himself to do or say, owing to self-consciousness and the rapid imagining of himself as he would appear in the eyes of others. At the same time, and as in part the same process, his sense of general responsibility diminishes. Now self-consciousness of this sort is one of the most intense and varied forms of consciousness. It depends upon the repeated interaction of mental systems subserving the ideation of various aspects of self with very numerous other systems, as they are excited. This interaction involves a very great amount of that physiological activity which we have found reason to regard as the immediate correlate of consciousness, namely, the establishment of new or relatively new nervous connexions. In such mental processes there is involved a greater proportion of unorganised, as distinct from organised nerve paths.[1]

In an early stage of drunkenness, the ruder, more primitive instinctive impulses, are apt to make themselves effective, because they are no longer controlled by the mass of partially organised mental systems that represent the social training and finer points of character of the individual. And in a further stage before consciousness is abolished, it is so much reduced that the person may repeatedly perform some such action as shaking hands, or the asking of some question, each time without remembering that he has gone through the same performance in the previous moment. Consciousness is, in fact, so low in intensity that on the morrow there may be no memory of even very extraordinary events that may have occurred during this period. During this stage all actions seem to be performed almost automatically, and the finer movements requiring accurate co-ordination and attention are no longer possible. In a slightly later stage consciousness seems to be completely absent, and even such instinctive actions as walking cannot be performed, owing to the inability to balance the trunk and co-ordinate movements; but the purely reflex element in walking, the rhyth-

[1] The excitement that so frequently accompanies the initial stages of the action of alcohol, is probably an entirely indirect effect, due to dilatation of the cerebral arterioles, and increased activity of the heart, together with the loss of sense of responsibility noticed above.

mic movements of the legs, is still possible, for if supported on either side the patient may walk fairly well. This is the initial stage of coma, and it is important to notice that even in a more advanced stage of coma, long after the abolition of apperception, attention and consciousness, some at least of the cortical neurons are still functional, those, namely, that form parts of well-organised mental systems; for it is possible, even in this stage, by loudly asking the patient's name, to elicit an appropriate response, and, as I have already pointed out, there is every reason to believe that speech always involves the activity of cortical centres. It must then be admitted that the abolition of function of the parts of the nervous system by these drugs, is effected exactly in the inverse order of the degree of fixity of organisation, of functional continuity of the nerve paths concerned.

Having regard then to the present state of the histological evidence and to the facts of the localisation of the action of curari and atropine, mentioned above, it seems highly probable that fixity of functional continuity depends upon the intimacy of the junctions between processes of the neurons and that these drugs act chiefly upon the junctions tending to abolish their conductivity for the nervous impulse. This view of the action of the drugs would afford a very satisfactory explanation of their analgesic action. For the increased resistance of the junctions between neurons, to the passage of the nervous impulse, means a strengthening on every hand of the physiological barriers. Hence these drugs tend to prevent that disorderly forcing of the barriers that seems to be the physiological correlate of painful feeling, so diminishing or abolishing both the pain and that widespread disorderly motor innervation, both visceral and somatic, which accompanies pain.

The phenomena of fatigue seem to point to the same conclusion. It has not been found possible to induce fatigue experimentally in such neurons as are accessible to direct observation, and the neurons of the lower reflex centres seem capable of great and long-continued activity without showing symptoms of fatigue. The functions of the nervous system seem to be affected by fatigue in just the same order as by alcohol and chloroform, *i.e.*, the more automatic the function, the more definitely organised the nerve path, the less readily is it affected by fatigue. It seems then probable that the waste products of metabolism like those drugs, tend to abolish functional continuity between neurons. And here again we find support for this view in the analogy of nerve and muscle; for it has been experimentally shown

that in excised tissues the junction between nerve and muscle is more readily affected by fatigue than either nerve or muscle.

This hypothesis finds further support in observations on the action of nicotine on the ganglia of the sympathetic nervous system. For it has been shown by Dr. J. N. Langley to be highly probable that nicotine abolishes the functional continuity between neuron and neuron in these ganglia, without markedly affecting the neurons themselves.

The action of these drugs then further tends to prove the correlation between consciousness and the passage of nervous impulses along new paths, *i.e.*, the establishment of new nervous connexions, for the possibility of the occurrence of both is diminished by them in an exactly parallel manner.

This argument remains good, though the fixity of continuity of nerve paths be not a function of the junctions of neurons, but depend upon some state of the cell bodies or their processes. Nervous impulses that are concerned in conscious processes pass through new or relatively new nervous paths, and in so doing improve their conductivity; and this process, the passage of the impulse along a new path improving its conductivity, is the immediate correlate of consciousness; and as by drugs and fatigue the process, be it called canalisation, organisation, "Bahnung," or the establishment of new functional connexions between neurons, is rendered impossible through increased resistance of the paths, so the possibility of the occurrence of consciousness is abolished.

PART III.

On the Relation of Consciousness to Other Existents.

In the preceding section I have but brought together data generally accepted by both psychologists and neurologists and have explicitly stated the logical inference from them that is more or less vaguely recognised by several writers. It is perhaps allowable to attempt now, in the light of this conclusion, to define more accurately the relation of consciousness to other existents and form a working hypothesis as to the part played by it in mental process.

The ground must first be cleared by a protest against a certain form of materialism that finds expression not only in such writings as those of Dr. C. Mercier, but also in those of some of the psychologists and philosophers who protest most loudly against materialism.

Our earliest and most frequently repeated sense impressions are derived from solid objects, by touch, and the sense of weight and resistance when we exert ourselves to move them; by sight through the light reflected from them; and the affections of our temperature sense are also generally referred to the presence of solid objects. Hence the natural man is inevitably a materialist in the sense that to him matter is the most real thing.

The first result of philosophic thought seems to have been the division of the universe into matter and spirit. Man's soul or spirit was regarded as in every way the antithesis of matter, and it was usual to applaud the one and heap opprobrious epithets upon the other. The former process was a useful step, the latter was a quite unnecessary diversion. When the mediate nature of our knowledge of matter became more fully recognised, matter was regarded as less offensive because less real. Then came a time when men busied themselves with a third conception—that of energy, and matter might have been relegated then to its proper place, but that physicists had come to look on matter in motion as the type of all energy. They committed themselves to the statement that action at a distance is impossible, a dictum that now seems so absurd, and began the attempt to formulate mechanical hypotheses for the description of all manifestations of energy. This attempt was attended with so considerable success in the case of certain forms of energy, notably light and heat, that it began to be believed that the hypotheses represented truths and that all forms of energy would ultimately be shown to be but forms of the motion of matter. This attitude of the physicists lent an immense stimulus to the natural materialism of all men, the metaphysicians and psychologists not excepted, with the result that the division between materialists and spiritualists became more acute than ever before.

It is by those who have least freed themselves from our primitive materialism and who have accepted most blindly the mechanical hypotheses of physicists, that the vastness and mysterious nature of the chasm, as they call it, between consciousness and the motion of molecules is most loudly insisted upon. We have already seen how the doctrine of the simple concomitance of consciousness has been set up from such reasonings. And the influence of this materialism is manifested very widely by psychologists of very different types. I have noticed already Tyndall's dictum that 'we have no organ that can bridge the gap'. Wundt writes: 'We can conceive how one motion may be transformed into

another, perhaps also how one sensation or feeling is transformed into a second. But no system of cosmic mechanics can make plain to us how a motion can pass over into a sensation or feeling.'[1] This sentence illustrates very fully the sort of assumption that I protest against, the assumption, namely, that we understand how one motion may be transformed into another in some fuller sense than that we have frequently observed this kind of sequence. A similar assumption seems to underlie the following sentence from an article by Bradley:[2] 'Between a process in the brain and a consciousness of energy there is really a gulf which is not to be filled up. You may know from experience that they are bound together, but, given the first, you could never have got to the second, and they remain in the end quite heterogeneous.' For it is implied here that in the case of other processes bound together you may know without experience that they are so bound together and may foretell the one on the occurrence of the other without previous experience of their relation. Similar statements are made by very many writers, and it is needless to multiply instances. What I wish to insist upon is that there is a certain crudity about all this. In insisting on the vastness of the gulf between consciousness and every other thing, writers seem to forget that the gap between all forms of manifestation of energy is really impassable by our minds; that when we profess to understand something of them, of their relations to one another, of their mutual inter-convertibility, all that we mean is that we have observed the conversions, so that we are familiar with them, we have measured with more or less accuracy the equivalent quantities of the different forms, and in some cases we have formulated mechanical hypotheses concerning them. Having so defined and described certain features and relations, we are apt to forget that our knowledge of and about these things is purely inferential, that our pictures of them and their interactions are but convenient mental symbols, that their nature is utterly mysterious, and that the chasm between every two kinds of energy is completely impassable by our minds. As Lotze writes:[3] 'Wir können leicht zeigen—dass in der Wechselwirkung zwischen Leib und Seele kein grösseres Räthsel liegt, als in irgend einem anderen Beispiele der Causalität, dass endlich nur eine falsche Einbildung bei diesen mehr zu wissen, die Verwunderung darüber erzeugt hat, dass wir hier nichts wissen'. And again Lotze writes,[4] 'alle Wechsel-wirkung zwischen glei-

[1] *Human and Animal Psychology*, p. 6.
[2] MIND, xi., p. 321.
[3] *Medicinische Psychologie*, p. 71.
[4] *Ibid.*

chartigen Gliedern stattfindet,' since, as he puts it, soul and matter are alike the shadows of the realities underlying them and between which the interaction takes place. But Lotze wrote under the influence of a flourishing materialism, and from the modern standpoint we must modify this dictum and say that consciousness and all manifestations of energy are alike but shadows of the realities underlying them, that hence 'alle Wechsel-wirkung zwischen gleichartigen Gliedern stattfindet'. For we can no longer divide the universe into soul and matter. We now realise that it is not and probably never will be possible to describe all forms of energy in terms of the motion of matter, and that in dealing even with light and heat the mechanical hypotheses have not been completely successful.

Further it is now recognised that the forms of energy that affect our sense organs are but a small selection, as it were, of those of whose existence we have indirect evidence, while, as Prof. Ostwald[1] writes, 'Matter is nothing but a group of various forms of energy co-ordinated in space, and all that we try to say of matter is really said of these energies'; and as Prof. Mach[2] has insisted, when we treat of matter mechanically we are regarding it from only one of many possible aspects.

In considering the relation of consciousness to neural process it is our business as psychologists to treat of consciousness objectively, or as Shadworth Hodgson[3] says, as 'an existent' in 'its dependent aspect of genesis and determination by nerve process' as distinct from 'its other aspect or character of a Knowing,' which must be left to be dealt with by metaphysicians. For 'It is this existent aspect of consciousness that is the object of psychology'. 'Psychology, therefore, has not to account for there being such a nature as consciousness, for this is the primary datum of all particular knowledge whatever, but, taking this nature as already known, to account for the genesis and subsequent history of it in individual living beings. And in order to do this, it is obvious that psychology must also treat the nervous system and process as realities or real existents, no less than consciousness.'

We have seen in the preceding part of this essay that consciousness seems to occur constantly in conjunction with the process of establishment of new nerve paths, of new functional connexions between neurons. If we reject the doctrine of the simple concomitance of consciousness, and regard it as playing some part, as performing some function

[1] *Science Progress*, Feb., 1896, p. 432.
[2] *Science of Mechanics*.
[3] *Brain*, vol. xiv., p. 8.

in mental life, as we have seen imperative reasons for doing, we must suspect some causal relation between these existents, we must believe that consciousness is one of the conditions of the establishment of the new connexions.

In all probability, this establishment of connexions among neurons, connexions which seem to be the physiological basis of retentiveness, consists in a redetermination of the grouping of atoms and molecules, so that they shall swing in new positions of equilibrium; the immanent energy of these systems then determines the course of transformation of the transitive energy of mental activity, just as the immanent energy of stable equilibrium of the molecules of a seed must be supposed to determine in part the course of the transitive energies of the growing plant that springs from it. But however this may be, if we ascribe to consciousness a causal relation with, if we attribute to it a part in, the determination of nervous connexions, then we must admit that in its aspect of an existent, the consciousness of another man has to each of us a relation similar to that of those forms of energy, such as magnetism, the ultra-violet rays of the solar spectrum, the Röntgen rays, and many others, which do not directly affect our sense organs but are only known to us indirectly by their interaction with those forms of energy that do directly affect us. The only difference seems to be that our means of appreciating its occurrence and estimating its intensity are still more indirect, more uncertain and more inexact.

And unless we assume that the mind either destroys or creates energy, we must believe that consciousness is subject to the law of the transformation and conservation of energy, we must in fact believe that it is a form of energy, and that it has its heat equivalent that may some day be determined with more or less accuracy. Have we not an indication of this in the fact that neural activity seems to produce fatigue rapidly in proportion to the intensity of the consciousness that accompanies it?

The legitimacy of this way of regarding this aspect of consciousness may be illustrated by putting a hypothetical case. There are good observers who believe that telepathy is a fact, *i.e.*, that the consciousness furnished by the brain of one individual, A, may, and sometimes does, affect the consciousness of another individual, B, without the intervention of the bodily organs of A. There is no ground for denying the possibility of this occurrence, and the only ground for believing it to be improbable is that it has not yet been satisfactorily proved to occur. Let us suppose, then, that some-

time in the future it should be proved to occur, and let us further imagine that the power of appreciating the consciousness of others in this way had become highly developed. Then if our appreciation were by means of some special sense organ, the relations of the consciousness of one individual to another would be strictly comparable, not merely, as at present, to the relation to us of the forms of energy indirectly appreciated, but to that of those forms that directly affect us.

While then psychology leaves consciousness in its aspect of a Knowing to metaphysic, it may assume as a useful working hypothesis that in its aspect of an Existent it is one of the forms of the manifestation of energy, that just as matter is but 'a group of various forms of energy co-ordinated in space,' so mind is a still more complex group of various forms of energy of which consciousness is one. We shall then regard mental activity, not as a mixed chain of neural and psychical events, but as a flowing network of cause and effect, consciousness forming a part of the strands, a part that is of very different importance in different kinds of mental activity. By adopting this hypothesis and thus bringing the relation of consciousness to other existents into line with the relations among the latter, we in no way diminish the mystery of the relation, we do but fill in a little more of the detail of the whole mystery of causality, as by all other advances in our point of view and knowledge of relations.

If we reject the simple concomitance view of consciousness and yet accept for mental process the principle of determinism, as is done by most psychologists, I cannot see that any other view of the function of consciousness than that here advocated can be held without involving irresolvable contradictions.

It would be idle to speculate further as to the exact part played by consciousness in establishing connexions among neurons, for we do not yet know in what those connexions consist, whether in some increase of the conductivity of nerve fibrils, the bringing of separate fibrils into continuity by the outputting of amœboid processes or otherwise, the modification of the substance intervening between neighbouring fibrils, perhaps by an orientation of polar molecules, or in some other physiological change.

The deductions from this hypothesis may be summed up shortly by saying that consciousness is the force that makes mind, that makes of neural processes experiences, that consolidates new reactions and thoughts into habitual mental processes, and habits into instincts and reflex actions. We act so or so, not because we are conscious now, but because we have been conscious in the past.

V.—THE DIALECTICAL METHOD. (III.)

(*Conclusion.*)

By Prof. E. B. McGilvary.

II. Let us now address ourselves to the contention that there is another kind of change in the method of the dialectic, a change from dependence upon contradiction into absolute indifference to contradiction. Mr. McTaggart says that the difference is greatest between the method employed at the very beginning of the process and the method employed at its end. In the categories of Being, neither the thesis nor the antithesis "is in any respect more advanced than the other, and neither of them can be said to be more closely connected than the other with the synthesis, in which both of them alike find their explanation and reconciliation. But when we come to Essence the matter is changed. Here the transition from thesis to antithesis is still indeed from positive to negative, but it is more than merely this. The antithesis is not merely complementary to the thesis, but is a correction of it. It is consequently more concrete and true than the thesis, and represents a real advance. And the transition to the synthesis is not now made so much from the comparison of the other two terms as from the antithesis alone. For the antithesis does not now merely oppose a contrary defect to the original defect of the thesis. It corrects, to some degree, that original mistake, and therefore has —to use the Hegelian phraseology—'the truth' of the thesis more or less within itself. As the action of the synthesis is to reconcile the thesis and the antithesis it can only be deduced from the comparison of the two. But if the antithesis has—as it has in Essence—the thesis as part of its own significance, it will present the whole of the data which the synthesis requires, and it will not be necessary to recur to the thesis, before the step to the synthesis is taken.

"But although the reconciliation can be inferred from the second term, apart from the first, a reconciliation is still necessary. For, while the antithesis is an advance upon the thesis, it is also opposed to it. It is not simply a completion

of it, but also a denial, though a denial which is already an approximation to union. This element of opposition and negation tends to disappear in the categories of the Notion. As these approach the end of the whole process, the steps are indeed discriminated from one another, but they can scarcely be said to be in opposition. For we have now arrived at a consciousness more or less explicit that in each category all that have gone before are summed up, and all that are to come after are contained implicitly. 'The movement of the Notion is after all to be looked on only as a kind of play. The other which it sets up is in reality not another.' And, as a consequence, the third term merely completes the second without correcting one-sidedness in it, in the same way as the second term merely expands and completes the first. As this type is realised, in fact, the distinctions of the three terms gradually lose their meaning. There is no longer an opposition produced between two terms and mediated by a third. Each term is a direct advance on the other before it. The object of the process is not now to make the one-sided complete, but the implicit explicit. For we have reached a stage when each side carries in it already more or less consciousness of that unity of the whole which is the synthesis, and requires development rather than refutation."[1]

A few pages farther on, we read: "We may draw several important conclusions with regard to the general nature of the dialectic, from the manner in which the form changes as it advances towards completion. The first of these is one which we may fairly attribute to Hegel himself, since it is evident from the way in which he deals with the categories, although it is not explicitly noticed by him. This is the subordinate place held by negation in the whole process. We have already observed that the importance of the negation in the dialectic is by no means primary. In the first place Hegel's Logic is very far from resting, as is supposed by some critics, on the violation of the law of contradiction. It rather rests on the impossibility of violating that law, and on the necessity of finding, for every contradiction, a reconciliation in which it vanishes. And not only is the idea of negation destined always to vanish in the synthesis, but even its temporary introduction is an accident, though an inevitable accident. . . . But this can now be carried still further. Not only is the presence of negation in the dialectic a mere accident, though a necessary one, of the gradual

[1] *Op. cit.*, pp. 124, 125.

completion of the idea. We are now led to consider it as an accident which is necessary indeed in the lower stages of the dialectic, but which is gradually eliminated in proportion as we proceed further."[1]

I have quoted thus largely in order that the issue may be clearly defined. My aim is not so much to controvert the expositor, but to expound his author and mine. But here, as in so many other instances, exposition is most effectively accomplished by a refutation of a counter-exposition. As every affirmation is also a negation, it is often well that what one denies should be brought clearly before consciousness, in order that what one affirms may be the more significant. As I understand Hegel, he affirms exactly what his commentator denies; and to understand the affirmation, it is well to understand the denial also.

It is my purpose to prove that in respect of the relation in opposition between the thesis and the antithesis, and in respect of the dialectical efficiency of the negative, there is really no change in Hegel's Logic from the very first triad to the very last. In order to do this, I shall attack the problem of the method of the initial triad, believing that a complete exhibition of the method there employed will be in itself a complete refutation of Mr. McTaggart's position.

As we enter upon this problem, we must recall what has been said about the double character of some of the categories, and about the so-called reconciliation of contradictories.[2] The true view of the movement, even in the categories of Being, is that the true dialectical negative of any thesis is not the abstract antithesis of that thesis, but it is the so-called synthesis of that thesis with the abstract antithesis; so that, if we are going to count steps, we may say that in one very important sense the dialectical movement is not triadic,[3] although, of course, if we consider the two aspects of the negative as two distinct steps in the process; that is, if we consider as one step the viewing of the negative as abstractly negative and as another step the viewing of it as concretely positive, we do have a triadic movement. But this whole matter of counting steps is a senseless procedure, an external reflexion; and the witticisms based upon it only show the shallowness of the views of the wits.

[1] *Op. cit.*, p. 134. [2] See January number of MIND, pp. 57-60.

[3] See III., 333, 334 (343-344), where Hegel shows that the movement may be considered tetradic or dyadic as well as triadic; but all such enumeration is "superficial and external".

Hegel's own testimony on this point is decisive. "It" (*i.e.*, the negation of any concept) "is a new concept, but one higher and richer than the preceding; for it has become richer by the negation or opposite of that concept. It thus contains its predecessor and more too, and is the unity of that predecessor and its opposite."[1] This is what he says in the Introduction to the *Greater Logic*. He says the same thing at the very end of this work,[2] where, having traversed the long road that leads from pure Being to the Absolute Idea, he reviews the course that has been accomplished, and pays particular attention to the method that has achieved the result. There he devotes some eight or ten pages to the full treatment of the relation between thesis, antithesis, and synthesis, or, as he there calls them, "the first," "the second," and "the third"; and he looks at this relation from many points of view. I cannot quote the whole passage, although any one who wishes to understand the method thoroughly should make himself perfectly familiar with what is there said. All I can do here is to quote in part: "The second" (*i.e.*, the antithesis), "which has thus arisen, is accordingly the negative of the first; and, to take anticipatory account of what follows, it is the first negative. The immediate" (*i.e.*, the thesis) "has, on this negative side, perished in its other; but that other is essentially not the empty negative, Naught, which is taken for the usual result of dialectic; but it is the other of the first, the negative of the immediate.[3] Thus it is determined as the *mediate*—in short, contains within itself the determination of the first" (*i.e.*, of the thesis). "The first is hence essentially *preserved* and *kept* in its other. To keep fast hold of the positive in its own negative, in the content of the presupposition, in the result—this is the most important thing in the knowledge that belongs to the stage of reason. . . . What is, accordingly, now present is the mediated, which at first, or when likewise taken immediately, is also a simple category; for since the first has perished in it, only the second is present. Now, because the first is also contained in the second, and the second is the truth of the first, this unity can be expressed as a proposition, in which the immediate is put as subject, and the mediate as its predicate. . . . The second category, the negative or mediate, is, further, likewise the *mediating* category. At first it can be taken as a

[1] I., 39 (41). [2] III., 328 (338), *seq.*

[3] The thought would be better expressed by the use of hyphens: "the other-of-the-first, the negative-of-the-immediate," and below, "the negative-of-the-positive".

simple category; but viewed as it is in its truth, it is a relation or an essential relation. For it is, indeed, the negative, but the negative of the positive, and includes the positive within itself. It is thus the other, not of something to which it is indifferent,—for then it would not be an other, nor would it be a relation or an essential relation,—but it is the other *by virtue of itself* (*an sich*), the *other-of-another.* Therefore it includes *its own* other in itself, and is, accordingly, as this contradiction, *the explicitly posited dialectic of its own self.* . . . The second, as contrasted with the first, is itself the determinate, *i.e.*, is distinction or essential relation. The dialectical moment in it consists, hence, in positing the unity that is contained in it. If, therefore, the negative, the determinate, *e.g.*, essential relation and judgment and all the categories that come under the second moment" (*i.e.*, appear as antithesis), "do not themselves, taken in their independence, already appear as contradiction and as dialectical, it is due to a mere defect of the thinking faculty, which does not bring its thoughts together. For the materials," adequate for the recognition of contradiction, "the opposite categories *in one relation,* are already explicitly posited, and present for thought. Formal thought, however, makes identity a law, and lets the contradictory content that it has before it be degraded into the sphere of presentative consciousness, into space and time; and there the contradictory is maintained in the reciprocal *externality* of coexistence or succession, and thus does not appear to consciousness to be in mutual contact. In its dealing with this content, formal thought lays down the definite principle that contradiction is not thinkable. In fact, however, the thinking of contradiction is the essential factor in conception. Even formal thought actually thinks contradiction; only, while thinking it, it refuses to recognise it, turns from it, and in that just-quoted principle betakes itself to abstract negation.

"The negativity just considered constitutes the critical point in the movement of the concept. It is the simple point of negative self-reference, the innermost fountain of all activity, of vital and spiritual self-movement, the dialectical soul that everything true has in it, and by which alone it is a truth."[1]

To bring out all the wealth of meaning that lies stored up in these cryptic utterances would take more pages than are at my disposal. But it requires no great space, and certainly no great penetration, to repeat some things that are

[1] III., 330-332 (340-342).

said by Hegel in the passage I have partly cited. The central thought of it all is this: *When we have reached any antithesis, we do not need to wait for a synthesis in order to reconcile that antithesis with its thesis; but the antithesis itself, viewed as speculative logic views it and not as abstract, make-believe, but impossible thought would view it, is already a synthesis of the thesis with the opposite of the thesis.*

In other words, the antithesis has two significations; or rather it may be regarded from two points of view. Looked at from one point of view, it is merely an abstract opposite of its thesis, having no *intimate* relation to it, living as a quarrelsome neighbour *beside* it, *separated* from it by an insurmountable wall of partition, through which only its objurgations can find passage. The law of identity keeps it bound down fast on its own side of the fence; the law of contradiction forbids its trespassing on its neighbour's premises; while the law of the excluded middle says that there is no fence between them. And thus this point of view opens before us a scene that is—illusion; "vanity of vanities, all is vanity". So we must betake ourselves to the other point of view, and see whether that will not display before us a substantive reality.

From this second point of view, the antithesis is no longer a virago living next door to the thesis, but is the indispensable, all-absorbing, husband-sublating wife, "the joy of its bosom, the plague of its life". The non-existent wall of separation no longer separates; it unites. The husband is seen to be only a member of a family, and the wife is the family.

But illustrations and all like aids to comprehension are only external. The matter must be understood in its native logical truth; and it seems to me that this truth is capable of being understood, and even of being expressed in language that is no more figurative than all languages must be. Let us try.

The negative of anything, as merely presentatively before consciousness (*als vorgestellt*), may appear to that consciousness with all suggestion of its intrinsic essential relation to its positive suppressed; only, one is tempted in such a case to appropriate Prof. James' happy word "sciousness," as a more faithful designation of such a mythical consciousness. When, for instance, I present to myself the image-idea of Naught, I may seem to have a mere vacuum as the only object before my consciousness, a vacuum which explicitly excludes the content of Being, and therefore has no Being in it. But this is a mere seeming. Because

if it is a merely presented Naught and not a thought Naught, not a conceived Naught, then it is not Naught *to me*, and therefore the presentation of it to me is not consummated. The desired result is achieved only when I *think* what is proffered, as being Naught; and to think it as Naught is to think it as negative of Being, as Non-Being; that is, Being must be *present as rejected* within the thought that thinks Naught. I must think what-it-is-not, and think Naught as *not* what-it-is-not. Naught is, then, the synthesis, in thought, of Being and rejection or negation, a synthesis of elements which, if they could exist out of such synthesis, would be pure Being and pure denial or abstract pure Naught. It takes, thus, only a little psychological reflexion to see that the thought of the negative is, as a matter of fact, always a thought that includes its positive as a moment within it; and it takes but a little logical reflexion to see that it must be so. The thought of the negative is, therefore, not a dead self-identity, but is a *synthetic unity, with the positive that it negates as a distinction partly constitutive of it;* whereas the thought of the so-called positive is never achievable except as merely a part-thought within the organic whole of a negative thought. But such a negative thought is not merely negative; in denying the positive, it not only posits itself but it also posits as a factor within itself the positive which it itself denies.

Now, even when "common-sense" rises to the height from which it can see that the positive is only a part-thought, it makes the mistake of supposing that the negative is only another part-thought, along with which the positive part-thought makes a whole by external juxtaposition. It is perhaps Hegel's greatest merit to have seen the insufficiency and utter inadequacy of any such supposition, and to have made this his insight the instrument for the reconstruction of the science of logic, for the rescue of dialectic from the fate of being a mere capricious iconoclast, and for making it the constructive principle of philosophy, seeing that it was the constructive and not the destructive principle of thought. He saw that what is only positive does not exist, except as a merely apprehended but not yet comprehended member in a concrete whole; he saw that what is only negative does not exist except as a whole not recognised as a whole; and he saw that what in truth really exists, and what alone exists, is the negative whole of thought, which, because negative, has distinctions; which, because a whole, has the distinctions within it and not without; and which, because not negating something outside of it

or an independent other, is truly positive. As already shown, this positive-negative whole, has, as its organic constitutive factors, the positive and negation. The factors, regarded in themselves, in abstraction from the whole in which alone they have being, are pure, *i.e.*, abstract Being and abstract negativity or pure Naught. But so regarded, they cannot be *thought*, and what cannot be thought cannot be logical; and so long as they are not thought, they have no dialectical character belonging to them. They get dialectical character only when they are taken up into thought; that is, when they cease to be mere unsubstantial ghosts, mere make-believes which are not, and become real thoughts that are. The dialectical movement is the change from what they are not and never were and never could be, but were supposed to be, to what they really are. Dialectic is the nullifying of a nullity that parades itself in the guise of an entity; but this nullity is not cancelled out of consideration. The abstractions that are shown by dialectic to be nonentities are shown to be such, only in being shown to be real organic factors in the one thing that can with any propriety be called real, that is, in negative-positive thought, or self-consciousness.

Now it is for the reason that the dialectical negative is thus negative because what it negates is *within* it,—it is for this reason that Hegel calls it "the other by virtue of itself, the other-of-another" ("das Andere an sich selbst, das Andere eines Andern"). It does not owe its otherness to the mere accident of being arbitrarily and externally contrasted with something to which it is indifferent, but it is an other because it has *in itself, as its own moment*, its other, by virtue of which it has the characteristic of otherness. It is itself the other-of-another, not what may be regarded as an other —of something independent.

Having thus shown what is the true logical view of the relation between the antithesis and the thesis, let us see whether this relation does not hold in the very first triad. As has already been shown, the first triad is not abstract Being, abstract Naught, *and* Becoming. But it is Being which has its reality *only in its negation;* which negation, *being concrete*, is the synthetic unity of Being and its opposite, Naught; in other words, that negation is Becoming. The thought or category, Becoming, is just the same thought as Non-Being when Non-Being is *thought*, and is not merely an imaged abstraction. As Hegel says, "in Non-Being the relation to Being is contained; it is both Being and the negation of Being expressed in one word; Naught, as it

is in Becoming".[1] Since it is thus a dialectical negative, which is richer than its thesis, including, as it does, both its thesis and the opposite of that thesis, it is "just as positive as it is negative"; and when thought of as positive we call it Becoming.

It would not be difficult to show that in any other so-called triad in the categories of Being, the antithesis, when thought, that is, when dialectical and not abstract, is already the synthesis. Take for instance the Finite, the false Infinite, and the true Infinite. The false Infinite is not really the antithesis in thought of the Finite; it is only by an unrealisable abstraction that the false Infinite has any being. To negate the Finite in thought one must think it as a moment in the Infinite; the Infinite cannot exclude it; hence the Infinite cannot be, in real thought, a false Infinite. And, again, the Finite cannot be thought except as a moment in the thought of the Infinite. Of course a finite thing may be discovered to consciousness by presentation without the presentation of the Infinite, for the Infinite cannot be presented at all. But this finite thing can be thought as finite, only as a distinction within a thought that thinks what the Finite is not, *viz.*, the Infinite.

But this relation between thesis and synthetic antithesis in these early categories is exactly the relation that exists in the categories of Conception (*Begriff*). Let us take Concept, Judgment, Syllogism, as example of the later triads. A negation of what usually passes for a concept is a judgment; that is, you cannot in thought negate such a concept without *ipso facto* judging. Nor can you ever judge without *ipso facto* negating such a concept. That is to say, unless the so-called subject and the so-called predicate are kept distinct, kept in the negative reciprocity of distinction, there is no judgment. But if this distinction is a *bare* negation, a mutual *exclusion*, there is no judgment either. The so-called subject and the so-called predicate are not in external juxtaposition. The true logical subject[2]—not the so-called logical subject—includes the predicate while denying it, that is, while distinguishing it from itself. As Mr. Bosanquet says, "Subject and Predicate in the actual judgment are really distinct, as a real identity from or in its differences". "You cannot affirm *without introducing a distinction or reference* into the content of the affirmation;

[1] I., 74 (79).

[2] The true logical subject in "synthetic judgments" is the all-inclusive unity of the distinctions named in the subject and predicate, and in "analytic judgments" it is the unity analysed.

and yet such distinction or reference, *being part of what is affirmed, and not a relation between what is affirmed and something else*, cannot, it would seem, be the essence of the affirmation."[1] The subject, the all-inclusive unity of distinctions, is the *negative* unity of these distinctions, not their dead identity. But because that negative is thus a mediating unity, in which the so-called subject and the so-called predicate of the "synthetic judgment" are brought together, it is the syllogism. To recapitulate, the negative of the concept, as explained above, is judgment; this negative of the concept regarded as positive, because not negative of something external to it, is the syllogism. Now the reader who has followed this very brief exhibition of the dialectic of Concept, Judgment, and Syllogism, will see that the method is exactly the same as that employed in the triad, Being, Naught, and Becoming.

We can now see what becomes of Mr. McTaggart's contention that the negative is only a necessary accident in the dialectical process, tending to disappear in the higher categories. We may traverse this contention in two ways. We may say truly that negation, far from being a necessary accident in the dialectical process, never appears in it—*meaning here by negation what Mr. McTaggart means by it;* for the dialectical process is never "from one extreme to another extreme equally one-sided".[2] And we may also say that negation is not an accident tending to disappear, because it is the very life of the process, and appears in the fulness of its power only when it has brought all things under its feet, as the All-in-All of the Absolute Idea.

It happens that abstract understanding puts over against the thesis some other would-be category, now in the relation of contradictory opposition, now of contrary opposition, now of subcontrary opposition; and what disappears in the higher categories is the particular definite way the abstract understanding has of relating a lower abstract antithesis with its thesis; and what takes its place is some other particular way. But what is permanent through all such changes on the part of abstract understanding, is the one way that dialectic has of advancing by negation from a lower category to the higher, in which alone the lower has being. "That by which the Concept accomplishes its self-advancement is the aforesaid negative that it has in it. This constitutes the truly dialectical feature."[3]

[1] Bosanquet's *Logic*, I., 82. The italics are mine, to emphasise what is here pertinent.

[2] *Op. cit.*, p. 134.

[3] I., 40 (43).

What we have already said will throw light upon another vexed question that concerns the nature of the dialectic. Is its method analytic or synthetic? Is the first category a box within which the others are all contained, or is it a grain of sand to which the others are added to make a heap? If this second form of putting the question fairly represents what is meant by the first form, the answer must be that it is neither analytic nor synthetic; for such analysis and synthesis are nothing but mere partition and aggregation. But if by analysis is meant the bringing to explicit consciousness (*Setzung*) of what a category is in itself (*an sich*), and if by synthesis is meant the enrichment of a category by another which is not an externally indifferent other, then the answer must be that the process is both analytic and synthetic. For in an organism, analysis and synthesis are not inverse processes, but are the same process. And by this I mean something different from what Mr. Bradley and Mr. Bosanquet mean when they say that there is no analysis without synthesis, and no synthesis without analysis. For what they mean is that a thought-whole does not crumble to pieces under the operation of analysis, but that the act of analysis can be performed and the result of analysis can be recognised as an analysis, only when in the same act these analytic elements are ideally held together in the whole, from the ideal disintegration of which they have arisen; and that an intellectual synthesis can take place and be recognised as synthesis, only when the synthetic result is not a homogeneous whole, but the distinctions unified are still kept distinct. All this is true *even in the ideal partition and aggregation of spaced objects conceived merely as mutually external.* Now what I mean by the co-operation, or rather by the identity, of analysis and synthesis is something very different from this. One cannot analyse the conception one has of any member in an organic whole without finding in that conception the conception of the other members and of the whole within which they are members; and this gives the analytic character to the operation. But at the same time these other members are truly *others*, and the whole is more than the part; and this gives the synthetic character to the operation.

Let us hear what the great dialectician himself says on this subject: "This is what Plato demanded of knowledge, that it should consider things in themselves, in their absoluteness; that is, partly, in their universality; but also, partly, that one should not stray away from them, and should comprehend them, not in their environments, instances, and

external contrasts, but that one should keep them alone before one's thoughts and bring to consciousness what is immanent in them. The method of absolute knowledge is in so far *analytic*. The fact that this method finds the further determination of its initial universal entirely in this universal alone, is the absolute objectivity of the conception,—and the method is the assurance of this objectivity. It is, however, just as much *synthetic*, since its object, determined in its immediacy as simple universal, shows itself as an *other*, by the very determinateness which it has in its immediacy and universality. This relation to a different something—a relation which constitutes the object in itself—is, however, not the same thing as is meant by synthesis in finite knowledge. Already by its quite as analytic character of being a relation *within a conception*, it is thoroughly distinguished from this latter synthetic procedure. This moment of *original self-diremption* (*des Urtheils*)—a moment as synthetic as it is analytic—by which this initial universal obtains out of itself its determination of being its own other, is to be called the dialectical moment."[1]

Let me give an illustration of Hegel's meaning by using what, at first, seems not to admit of serving as such. What is the relation of a plane triangle to the space outside of it? Of course, if merely looked at, the triangle is seen to be outside of the space that is outside of it. Considered as mutually external, neither can by analysis be derived from the other. But instead of merely looking at the triangle, *think it*, that is, bring it under conception, and proceed to examine logically the concept you have of it. Logically, the concept of it is the concept of a plane-figure-bounded-by-three-straight-lines. That is to say, we find as an element held logically within the organic unity of the concept "triangle," the concept "line". Now examine this concept "line"—again avoid merely looking at it in a passive gaze—and you will find that it is the concept of limit-between-two-surfaces. That is, the concept "surface" is an element held logically within the organic unity of the concept "line". Again, examining this concept "surface," you will find that it is the concept of limit-between-two-solids; so that, here again, we find that the concept "solid" is included as an element logically within the concept "surface". Thus we find that the tridimensional space, circumjacent to a plane triangle, is an *analytic element* in the very concept of that triangle. But it is no less true that this tridimensional space is the *inclusive*

[1] III., 326 (335-336).

concept, in which the concept of the triangle in its turn is an element ; that is, this tridimensonial space, when thought, is *synthetically* related to the triangle, having the triangle as an analytic element within it. We do not, and cannot, think tridimensional space as an element in a quadridimensional space. We think it as a moment in the concept of one or more of its dimensions, and think it as the total concept including its dimensions as its moments. The analysis of the concept of a dimension gives tridimensional space as a distinction within it, but this distinction is at the same time the synthesis of this dimension with the other dimensions. But such synthesis and such analysis are not juxtaposition of mutually external pieces and partition into mutually external segments. The analysis is the recognition of the distinctions as moments within an organic unity ; the synthesis is the recognition of each distinction as itself *the whole within which all other distinctions, themselves wholes including it, are included.* No distinction is an ultimate distinction incapable of analysis. Think it as an ultimate distinction, and, behold, in that very thought it becomes a synthetic whole subsuming under it that whole in which you try to consider it as an ultimate element. *In conception, then, subsumption is reciprocal.* The thought-movement,—that is, the dialectic,—by which we go from a distinction within a unity up to that unity, is a movement outwards, which at the same time is not a movement outwards, but a permanent abiding within the point of departure. "Dialectic is the *movement outwards, that remains within,* a movement in which the one-sidedness and limitation of the categories of the understanding shows itself to be what in reality it is, namely, its negation." [1] An analysis of a concept as it is in itself, which results in a synthesis of it with a true other ; a synthesis of a concept with its other, which is not an absolutely independent other, but is an other within that concept—this is the dialectical process, this is the dialectical method, which, beginning with a seemingly poor, ultimate, unanalysable concept, reaches the richest concept "without introducing any element from without".[2] "Every new step of the *process outward out of self,* that is, of *further determination,* is also a *process within self.*" [3]

[1] *Encyclopædia,* § 81, Werke, vi., 152. Prof. Wallace's translation, "indwelling tendency outwards," of Hegel's "das immanente Ausgehen" not only robs the sentence of its strong paradoxical flavour, but fails to express the vital thought that Hegel expressed in the original.

[2] I., 39 (41). [3] III., 339 (349).

In closing this examination of Hegel's dialectical method, there are two remarks I wish to make. One of them is that the relation of Hegel's negative to the negative of formal logic is often misstated. It is not true that formal logic deals with a negative that is absolute, with a contradiction that cannot be gainsaid; while speculative logic deals with a negative that is relative, with a contradiction that has no backbone and can be bullied into retractation. The truth is, that the dialectical negative is the only negative that can be thought, and it is an absolute negative. We often make ourselves believe that denial can be a relegation of the denied into another universe than our own, or at least that it can be the expression of the logical non-existence for us of what is denied. And we more often make ourselves believe that contradictions cannot but logically annihilate each other. It is one of Hegel's good offices to philosophy to have pointed out that a negative thought not only *can*, but *must*, be absolute, while no negation can be "bare," and that contradictions are not only thinkable, but that where there is thought, there is contradiction—however, a contradiction *transcended*. The contradictories of formal logic are abstractions, good enough in their own place; but the law of contradiction is not a law of *active* thought; it is merely the law that expresses the relation that exists between two organic elements in thought, when we insist on not regarding their organic character. It is the law that controls the external relation of objects of presentative consciousness, when thought as presented; it is not a law that controls the relation of objects of intellectual consciousness, when thought as intellectual and timeless. In so far as any man thinks, and not merely passively "takes it in," he has transcended the law of contradiction and is under the law of dialectic.

The second remark is, that it follows as a consequence of the *analytic* character of dialectical synthesis, that there is no *external necessity* in the world of thought, in the eternal world that lies behind the world of time and space and is the ground of that temporal and spatial order. In the logical universe, every distinction is itself the whole by virtue of the fact that the other distinctions are logically within it. Every distinction is a monad, a whole logical universe within itself, entirely self-active, knowing no restraint or compulsion from an independent, external source; for everything that is other than it, is an other *in* and *for* it. But that other in and for it is not a *dependent* element in the sense that it for its part looks outside of itself for its ground of being. As

it is a logical element, it is likewise itself the whole, in its turn sublating what sublated it.

How many wholes, then, are there? There is one whole, and there are many wholes; for the hard and fast line of difference that runs between "one" and "many" is a line that can be drawn only when dealing with elements that are thought under the relation of mutual externality. The logical universe can, therefore, with no more propriety be called a monistic universe than a pluralistic one; nor, on the other hand, is there any more propriety in calling it pluralistic than in calling it monistic, except in the polemical interest of denying an asserted one-sided monism. Neither a pluralism with no real logical unity, nor a monism with no self-active distinctions, is compatible with the laws of thought. And those who read Hegel's monism into a system in which there is no liberty except the one *single* liberty of the one *single* whole, make Hegel do violence to the fundamental law of the totality of each logical distinction, a law which he himself made central within his system. The truth is neither a Spinozism in which the distinctions disappear, nor an idealistic monism [1] in which the distinctions are efficiently operated by a free comprehending unity, not an atheistic pluralism [1] in which there is no logical ground of interrelation between the distinctions. Mere monism and mere pluralism are transcended in the view that emphasises the logical necessity of the free distinctions, and the synthetic as well as analytic character of each of these distinctions. In fine, the truth is neither an absolute *One* at the expense of the Many, nor the separately independent *Many* at the expense of the harmonising unity of the One; but it is the *unity* of the One and the Many, a logically free One of the logically free Many, which are themselves, each, the One.

And it is just here that logic makes room for ethics. For ethics is possible only when there is interrelation between free self-active centres of consciousness, each recognising itself and others as members of one commonwealth of souls, in which each has rights and duties.[2] Each has rights, be-

[1] I do not mean to imply that all idealism is monistic or that all pluralism is atheistic; all I say is that there is a kind of idealism that is monistic, and a kind of pluralism that is atheistic; and of both I say that they are logically inadequate.

[2] I wish here to acknowledge my indebtedness to Professor Howison of the University of California for the great help I have received from him on this point,—as indeed on very many others brought out in this series of papers,—although the use I have made of this truth is not such as he would greatly approve, denying as he does any ethical value to Monism.

cause each is the end toward which all work; each has duties, because each works toward accomplishing the end of others. Each has rights, because each is the synthetic unity within which the others function as moments; each has duties, because each functions as a moment within the unity of the several others. No logic that does not provide for such a relation between self-active members of a system is ethical; no ethics that does not provide an intellectual and dialectical basis for such a community is logical. And as the Hegelian ethics rests upon the dialectic, and as the Hegelian dialectic provides satisfactorily for ethics, the charge often made that Hegel merely asserts freedom for the individual without vindicating it, is seen to be baseless. The Hegelian dialectic is adequate to all scientific needs—ethical needs among others—because all scientific needs are but expressions of dialectical necessity; but the Hegelian dialectic that is adequate, is the *Hegelian* dialectic, not the *pseudo-Hegelian* dialectic, which occupies and concerns the critics. Of course in claiming adequacy for the Hegelian dialectic I do not claim that it is the last word in philosophy, or that as Hegel worked it out it is inerrant. All that is meant is that this insight into the fundamental relation of the One and the Many is the insight that solves the true problem of present-day thought, the problem of making an ethical world intelligible and an intelligible world ethical.

VI.—CRITICAL NOTICES.

Theoretical Mechanics: an Introductory Treatise on the Principles of Dynamics. By A. E. H. Love, M.A., F.R.S., Fellow and Lecturer of St. John's College, Cambridge. Cambridge, 1897. Pp. xiv., 379.

The study of matter, as conducted by the physicist, deals, broadly speaking, with two fundamental problems. These are concerned, respectively, with its geometrical constitution and with the laws of its motion. The two are closely akin; for while we deal, in discussing the first, with the general nature of the spatial configuration of matter, we seek, as an answer to the second, for the relation between configuration and motion, or, what comes to the same thing, for the relation between successive configurations.

The present work, being engaged on Newtonian Dynamics, is occupied primarily with the second of these problems. It is, however, a rare merit in Mr. Love that he emphasises the connexion of his problem with that of the constitution of bodies; and this remains a merit even if, as will be suggested below, the connexion should be not quite as he states it. But we must first examine what is said as to the laws of motion. In doing so, it will be well to avoid, as far as possible, the specially mathematical portions of Mr. Love's book, and confine ourselves to those fundamental points which are of interest to the philosopher.[1]

The discussion of the postulates of Dynamics assumes, almost necessarily, a more or less historical form. This is owing to the fact that Newton's laws, though still found mathematically adequate for most purposes (if not for all), were stated by him in a form which requires, for its validity, a difficult process of explanation and interpretation. I cannot do better, on this point, than quote Mr. Love's Preface.

"The foundations of Mechanical Science," he says, "were laid by Newton, and his achievements in this department constitute perhaps his most enduring title to fame. Later writers have developed his principles analytically, and have extended the region of their application, but, in regard to the principles themselves, they have acted the part of commentators. Nevertheless

[1] The portions which will be mainly discussed are the following: Preface, chaps. i., v., viii., xiii., with the notes at the ends of chaps. v. and viii.

we may trace a tendency in modern investigations, which is of the nature of a gradual change in the point of view: there is less search for causes, more inclination to regard the object to be attained as a precise formulation of observed facts. On another side there is an important respect in which modern writers have departed from the form of the Newtonian theory. The philosophical dictum that all motion is relative stands in pronounced contradiction with Newton's dynamical apparatus of absolute time, absolute space, and absolute motion. It has been necessary to reconsider in detail the principles, and the results deduced from them, in order to ascertain what modification would be needed to bring the theory of Rational Mechanics founded by Newton into harmony with the doctrine of the relativity of motion."

The problem thus stated is one of essentially philosophical importance. Owing, however, to the fact that "the purpose of this book is didactic," as we are told in the Preface, a lengthy critical discussion of controversial points would be out of place. It is sometimes necessary, therefore, to supply from elsewhere the grounds for rejecting opposite theories. Acknowledgment is made to the works of Kirchhoff, Pearson and Mach, and a comparison with these is often useful in supplementing an unavoidable brevity. At the same time, Mr. Love appears entirely free from the anti-metaphysical dogmatism which mars the work of Mach, and still more of Prof. Pearson, while he has made an almost immeasurable advance, in philosophic correctness, on such hitherto standard works as Thomson and Tait's *Natural Philosophy.*

The first point on which we must be clear is the relation of Rational Dynamics to experience. I do not mean the question whether, as Mr. Love says (pp. 1, 2), the postulates of Dynamics are derived from experience, or are in part *a priori;* I mean the question whether, in view of the apparent inaccuracy of our results, we are to hold the postulates rigidly true, and only our knowledge of particular circumstances at fault, or whether, as in Economics for example, we shall regard our postulates themselves as approximations, in which case we shall test our abstract theory primarily by self-consistency. This question is one of great importance; for if the inaccuracy lies in the postulates, the true laws may bear very little logical affinity to those at present current, and the latter, therefore, may sink from philosophical principles to mere mathematical first approximations. It may be, for example, that a complete mechanical theory in terms of the ether would use different laws of motion from Newton's, and would exhibit Newton's laws, like Coulomb's law of the inverse square in electrostatics,[1] as subordinate results of a mathematical deduction.

That the traditional laws of motion are not ultimate principles

[1] See Hertz, *Electric Waves*, p. 225.

of nature, appears to be the view of our author. He speaks of an "abstract logical theory of Mechanics," and says: "The test of the validity of a theory of this kind is its consistency with itself, the test of its value is its ability to furnish rules under which natural events actually fall" (p. 2). More explicitly he says (p. 92): (Rational Mechanics) "may be regarded as a purely ideal system, and its validity is unaffected by the question whether it has or has not any relation to the observed motions of natural bodies. . . . The objects of which it treats are pure objects of thought." [1] I cannot help thinking that this statement, if it does not actually go too far, is at any rate liable to mislead the student. Mechanics is not, like the rules of chess, a purely conventional system, in which the relation to experience is wholly irrelevant; it cannot be idle to ask whether its postulates are true or false. The *objects* of our Science—rigid bodies, perfect fluids, particles, etc.—may perhaps be regarded as fictions, but even then, not only do the postulates remain either true or else in need of correction, but it would seem that even the matter to which we apply them must, in the last analysis, be regarded as having some exact and precisely definable nature. Probably, however, Mr. Love's statement is intended not to deny this, but only to point out that he is concerned with the deductive portion of the Science, not with the inductive verification of its postulates as applied to actual matter, and that the deductions remain logically valid if the postulates are found inaccurate.

As regards the formulation of the laws of motion, the author follows Mach in regarding them as constituting, essentially, the discovery and definition of mass (p. 101). He also agrees with Mach in considering it as the object of Dynamics to furnish a description of motions. But he adds that this description involves the notion of the mutual actions of bodies (p. 85). It is possible, of course, to define mutual action in a way which does not obviously involve anything causal, and this is no doubt intended in the definition given (p. 86). But can we not combine the essentials both of the causal and of the descriptive view, by stating it as the problem of Dynamics to find a universal relation between configuration and motion? By this means we avoid the supposed obscurities of the causal view, as well as the apparent arbitrariness of the descriptive view. If *mere* description were in question, it is difficult to see what ground we should have for belief in general laws. The essential problem, one might say, is this: from a given condition of a material system, to calculate the condition at some other time; and this problem, since it involves the validity of general laws, is not included in mere description. Inference from past to future, or from one body to another, at once transcends the powers of mere description; but this fact is hidden, I think, by a wrong view of the so-called uniformity of nature. Mr. Love appears, like Prof. Pearson and many other writers,

[1] *Cf.* also Critical Note, pp. 141-4.

to regard this uniformity as consisting in the recurrence of actual sequences of events (*cf.* p. 1). But such recurrence is never more than a gross approximation: no condition of the Solar System, for example, is ever exactly repeated. Nevertheless the uniformity of nature is not approximate, but consists in the permanence of laws, by which, whatever the condition of the Solar System, its next condition is rendered determinate. It would seem preferable, therefore, to regard the connexion of configuration with motion, rather than mere description, as the problem of Dynamics, and as the problem which the laws of motion render soluble.

The statement of the postulates of Mechanics is naturally much influenced by the doctrine of the relativity of motion, and consequently emphasises and amplifies the third law. It contains two points which are not obviously to be found in Newton's statement, but seem none the less essential. The first is that "all changes of motion arise from the mutual actions of the particles of bodies," and the second is that the mutual action is in the line joining the particles. The former is contained in the wider statement, which I should prefer to substitute for it, that the accelerations of particles depend upon configuration. The latter is often supposed to be implied in Newton's third law, but whether it be a gloss or an addition, it is important as specifying, to a certain extent, the manner in which acceleration depends upon configuration. It appears almost self-evident, and could, I believe, be proved, as soon as we make the proviso—required for the truth of the parallelogram of forces[1]—that the mutual action of two particles is independent of all other particles.

In chapter viii., on Work and Energy, the postulate is introduced (p. 136) which I should prefer to place at the threshold of Dynamics, the postulate, namely, that change of motion depends on configuration, or that all forces, when the circumstances are thoroughly analysed, will be found to be positional forces. If this postulate is to be always true, we must assume that, in an apparently rigid body, there are other motions besides those of the body as a whole. This involves, as Mr. Love points out (p. 137), that the body is not really rigid, and suggests, what on other grounds is highly probable, that the body has a molecular structure. In this suggestion, the author finds a difficulty, which is discussed in a Critical Note (pp. 141-4). On this point, I cannot help thinking that a mistake has been made. For discontinuity, surely, is of the essence of the Newtonian theory, and constitutes its chief distinction from the Dynamics of the ether. Thus Maxwell says (*Electricity and Magnetism*, § 529):—

"We are accustomed to consider the universe as made up of parts, and mathematicians usually begin by considering a single

[1] The parallelogram of forces cannot, I think, be deduced, as Mr. Love deduces it, *merely* from the definition of force as a vector, for it involves the physical axiom of the mutual independence of forces.

particle, and then conceiving its relation to another particle, and so on. This has generally been supposed the most natural method. To conceive of a particle, however, requires a process of abstraction, since all our perceptions are related to extended bodies, so that the idea of the *all* that is in our consciousness at a given instant is perhaps as primitive an idea as that of any individual thing. Hence there may be a mathematical method in which we proceed from the whole to the parts instead of from the parts to the whole."

What this quotation illustrates, and what I wish to make clear, is, that the Newtonian method is based upon the conception of a *particle*, as the ultimate self-subsistent element of matter, and that this conception is essentially discrete. The conception is not, therefore, it would seem, correctly described as that of "bodies, as continuous and made up of particles" (p. 141). Continuity, in the strict sense, excludes particles. To conceive the particles as "continuously filling the volume within the surface of the body" (p. 93) does not really remove the discreteness of the conception. The continuous filling must be regarded as accidental, so to speak, not as essential. To give it up, therefore, when we are regarding matter as made up of particles, is to make no fundamental change in our point of view; on the contrary, the continuous filling must itself, if it is to be accepted, be a result, not *a priori* probable, of experience as to various hypotheses. The natural result of Newtonian philosophy, as appears historically, is the discrete Dynamism of Kant, or rather of Boscovich. The only motive, apparently, for regarding bodies, in Newtonian Dynamics, as having a continuous structure, is one of practical convenience. That continuity of structure is in any way logically bound up with Newton, I cannot see; rather I should say that *discreteness* is the natural outcome of regarding matter as consisting of particles whose interactions are mutually independent. The molecular theory, therefore, though it undoubtedly introduces practical difficulties, seems, logically, rather of the nature of a simplification and development of the Newtonian stand-point.

Even admitting, however, that the difficulty is rather practical than logical, the way of escaping from it, as sketched by Mr. Love (pp. 142-4), is highly instructive and important. It consists in the adoption of the energy method, in which everything is deduced from the expressions for the kinetic and potential energy of the whole system. Although this method, historically speaking, has been derived from Newton's laws of motion, it may well be, as our author appears to suggest, that it will ultimately be found derivable from less special postulates, and that these will contain the true and accurate laws of motion. This question is connected with the question discussed above, whether, namely, the laws of motion are to be regarded as accurate, or only as first approximations. The suggestion of the latter alternative constitutes, to my mind, one of the greatest merits of the present work.

There remains only one question to discuss, but this is perhaps the most important of all—I mean, the relativity of motion. Mr. Love, like Mach and most modern writers, sees no insurmountable difficulties in the way of reconciling this relativity with the laws of Dynamics. The best way of discussing the question is in connexion with the law of gravitation, where the difficulties are most apparent. This is the course adopted by our author, who devotes his final chapter to a very thorough discussion of the matter. Briefly put, the course of his argument is as follows: Position, motion and acceleration are, by their very meaning, relative. Wherever we speak of the velocity or acceleration of a particle, therefore, we must specify the set of axes relative to which they are estimated—the frame of reference, as it is called. Now as long as our questions are purely kinematical, the frame of reference is indifferent. But when we come to kinetics, we have to chose what have been called "kinetic axes".[1] For, while accelerations depend upon the frame of reference chosen, we have declared, by the third law of motion, that accelerations always occur in pairs. We cannot, therefore, choose as our origin any particle of the material system considered, since this particle would then have no acceleration, and the necessary reciprocity would be lost. Generally speaking, we cannot choose as origin any particle having any dynamical connexion with the system considered. Practically, we may evade the difficulties by referring the motion to a very distant system, as when motions within the Solar System are referred to axes fixed relatively to the fixed stars. But if we wish to make gravitation universal, we shall have to abandon this practical makeshift. For this, and for the more general law that accelerations occur in pairs and are inversely proportional to masses, we must choose as origin, for the motion of an independent system, the centre of mass of the system, or some point moving uniformly relatively to the centre of mass. Through this origin we have *one* natural axis of reference, namely, axis of resultant moment of momentum (p. 365).

Now to this statement, if regarded as a satisfactory account of the postulates of Dynamics on the basis of relative motion, two objections may be made. The first is, that *two* natural axes of reference are required, and that, as appears from the Dynamics of a rotating system, the second cannot be arbitrarily chosen, and will have to be fixed, not in relation to any matter in the system, but in relation to what can only be regarded as empty space. This is the point of Newton's apparently unanswerable remarks on centrifugal forces.[2] The second objection is even more funda-

[1] See "Newton's Theory of Kinetics," by Mr. W. H Macaulay; *Bulletin of the American Mathematical Society*, July, 1897. This expression is not used by Mr. Love, but it affords an abbreviation which is often convenient.

[2] In the scholium which precedes the laws of motion.

mental. The centre of mass, which was to form our origin, is itself a deduction from the laws of motion, and presupposes a method of comparing mutual accelerations. It cannot, therefore, be supposed known in enunciating these laws. Masses are compared by means of the law that the mutual accelerations of two particles are inversely as their masses. But this law presupposes a frame fixed independently of the particles in question. Hence it is a vicious circle to deduce the appropriate frame from an assumed knowledge of the relative masses. The only way in which, theoretically, this difficulty could be evaded, would be the discovery of some body (like C. Neumann's "Körper Alpha") dynamically unrelated to the system considered. But dynamical relation to all other matter, as the law of gravitation illustrates, is of the essence of all matter. Hence the difficulty would seem *theoretically* unsolved. Practically, of course, it is evaded, for most purposes, by the very small degree of dynamical connexion which is assumed to exist between the Solar System and the fixed stars. But the relativity of motion, as Mr. Love says, is a *philosophical* dictum, and philosophy has a right, therefore, to examine its consequences from a philosophical standpoint.

The difficulties in the way of the old Newtonian absolute view are, as every one knows, almost, if not quite, insuperable. Nevertheless, in some sense, it reappears with extraordinary persistence. It is to be observed that a frame determined, as Mr. Love expresses it, by dynamical considerations (p. 365), may yet involve absolute space. Suppose, for example, that nothing existed outside the earth. The proper deduction from Foucault's pendulum would be, then as now, that the earth rotates with a certain definite angular velocity. The axes relative to which the rotation takes place are fixed, in a sense, by relation to the earth: we can assign their relation to the matter of the earth at any specified moment. Nevertheless, the assumed rotation is meaningless unless we regard our axes as fixed by a constant relation to empty space. For the axes themselves, being not matter, nor fixed by a geometrical relation to matter, are nothing but mere empty space; the assumed rotation, therefore, is a rotation relative to nothing. This brings out an important point: for truly relative motion, our axes must have to *some* matter a relation which is not only definable, but constant.

Although there appear to be unremoved difficulties in the relativity of motion, Mr. Love has certainly chosen a lesser evil than Newton's absolute motion, and has succeeded, by a candid inquiry, in presenting the prevailing view about as well as it is possible to present it. His last chapter ends with some admirable remarks on the measurement of time; but as this is a less debatable topic, I shall not discuss it here.

The present work is undoubtedly, from the point of view of first principles, the best English book on Dynamics; its free sceptical discussions are a most refreshing contrast to the scornful dog-

matism of most mathematicians; and its views, even where they cannot ultimately content the philosopher, seem for the present, on most points, the best with which the Science of Dynamics is at all possible.

B. RUSSELL.

Johannis Wyclif Tractatus de Logica.[1] Now first edited from the Vienna and Prague MSS., by MICHAEL HENRY DZIEWICKI. 3 vols. London: Published for the Wyclif Society, by Trübner & Co., 1893-99. Pp. xlvi., 241; xlvi., 236; xxxviii., 239.

M. H. DZIEWICKI has merited the thanks of the whole Philosophical World for his learned, important, and painstaking labours. If it has been too much left to foreign scholars like Drs. Lechler, Buddensieg, Beer, Loserth, the present Editor, and others, to do Wyclif justice, we ought at least not to fail of giving their work its due. No more ought we to fail of giving Wyclif his due, so that, while Wyclif be proclaimed a great philosopher, our philosophers may not be content to remain in ignorance of his writings, of which the world has been in possession for 500 years. In his careful and sympathetic Introduction to the first volume, Dziewicki puts in a plea for deductive logic which is not unneeded before the extravagant claims of Induction at the present day. For Induction is, after all, but a provisional process whose results can take the form of Causal Laws only under the lead of deductive reasoning. We have, in Induction, to search out the universal as it lies hid in the isolated cases that may be presented, but the enumerative view of Induction which the Scholastics took was one which never could arrive at Universal Truth or knowledge. The Inductive and deductive aspects are now known to be aspects that relate to the order in which we view reality, and do not bespeak antagonism. Dziewicki also shows a justice to Scholasticism that we have too often missed in Philosophical writings of our time. Current thoughtlessness regarding Scholasticism allows its spirit of speculative depth and inquisitiveness, its unmeasured confidence in the powers of the intellect, its transmitted wealth of principles, elements and terms, all to pass into the inheritance of to-day with rarely a word of grateful acknowledgment. Not so Dziewicki, who is keenly alive to the merits of the Scholastics. He points out, however, how the *Logica* of Wyclif differs from the works of the Scholastics, in that it leaves aside argumentation and Syllogisms, vital as these seemed to Scholasticism, the reason being that Wyclif wished to counteract Nominalism, and give a Realistic turn to Logic. Nor did he wish to counteract Nominalism only, but also certain theological tendencies of his time. Hence this

[1] Read before the Aristotelian Society, on 6th June, 1898.

"Doctor Evangelicus" styled his work, in the Proemium, the *Logic of Holy Writ*, meaning thereby to point to the Holy Scriptures as rule of faith and right reasoning. A true instinct led the Schoolmen to place Logic in the forefront of philosophical training, seeing that thought, as the instrument of philosophy, finds the primary attention it deserves in Logic as the theory of thought. The realistic tendency of Wyclif is early disclosed in his remarks on Identity at the close of the second chapter. Analogically, says Wyclif, all things, God and His creatures, are identical (*Omnia sunt idem in entitate*, p. 10). Dziewicki duly notes the extreme character of this realism, which goes far beyond the position of those who hold to analogical identity, but regard such identity as not real because analogical. God is, with Wyclif, identical with the creature in respect that they are both being. But Wyclif has his answer for those who think he identifies God with the creature, and makes substance to be accident. His answer is, in effect, that *that which* is being in the case of God cannot be logically concluded to be the same with *that which* is being in the creature. Wyclif is marked by the usual Scholastic subservience to Aristotle in the matter of categories. Though the Aristotelian scheme of Categories has met with severe enough criticism from the Port Royalists, Kant, Lotze, Mill and others, yet these critics have, in turn, been called to account, with more or less validity and force, by writers like Baynes, Mansel, Grote and Bain. What we still wait to know is how these categoric principles determine thought, the while they do not of necessity come into our consciousness. Much that Wyclif held in common with our modern logical treatises may be passed over without comment, as well as much that partakes of the inevitable tendency to mere verbal quibbling. We must be content to note some characteristics of the work, so replete with interesting material even for those of us who *are* moderns, and cannot will things otherwise.

When we come to "Logicae Continuatio," we find it laid down in the first chapter that truth is the correspondence of a proposition with that which it primarily signifies, that is to say, with its most general signification. "Unde Aristoteles," says Wyclif, "amplians hoc nomen, *res*, ad significandum quamcunque huiusmodi veritatem, dicit quod in quantumcunque res est vel non est quam proposicio primarie significat, est ipsa vera vel falsa" (p. 78). Here we soon again become confronted with the undue Realism of Wyclif, who holds that, though two men are two animals, yet they do not make four, but three, and on this wise. The number three is made up of the two individuals *plus* the Universal Animal or Man, present in, yet distinct from each. This suggests the famous Thomist theory of Hypostasis as something real or superadded to the substance, according to which, however, the real difference yet obtained only between singulars, and the Universal Man, conceivably present in all, was no more than an abstraction. Wyclif's Realism is not

remarkable for moderation. What wonder that Logicians of to-day hold all real existence to be necessarily singular, and yet reject Realism, the general notion not being, to them, of any metaphysical significance?

Passing to the second Tractate, we find Difference and Otherness discussed. The mysterious "*that which*"—persisting through all changes—again figures largely in the discussion, in which, as our Editor remarks, many Hegelian ideas lie all undeveloped, sealed up in Scholastic phraseology from the mind of Wyclif himself. In Wyclif's treatment of the Contingent, it is maintained that the ultimate cause of all indetermination is God —*omnis talis indeterminacionis causa ultima erit deus* (pp. 168-169). We are so far with Wyclif as to say that it is precisely as souls do choose the God-centred life that they rise to indeterminate heights of freedom and power of choice. The Contingent Truths depend, with Wyclif, on the human will (*possunt principiari a voluntate humana*, p. 169) as their immediate cause. One instinctively recalls in such connexion what Prof. James has urged, in dealing with the dilemma of Determinism, that somewhere for us *possibilities* exist, and form a part of truth, this world of ours not being one of simple unbending fact.[1] In strict keeping with this postulation of possibilities, which have so great relation to the future, Wyclif lays it down that all indetermination depends on things to happen in the future. The freedom to which even to-day we aspire is a freedom hewn out of the actualities of the present under the pattern of that ideal freedom which lures us on by the possibilities it presents to the perfected life. Our freedom is therefore real, but it is always becoming, and has never wholly attained, neither is already finished and perfect. It is the freedom of those who not only choose the ideal good, but who have also gained toil-won ascents of actual freedom. Necessity and contingency are not absolutely opposed, in the view of Wyclif. It is, however, vain to try to evade the lines being sharply drawn between these two. Wyclif's doctrines of necessity were anathematised by the Council of Constanz in 1415, for the temper of that time was such at least as could find no delight in bold paradox.

We now pass to something of epistemological interest, namely, the senses in which knowledge is to be understood. A more modern treatment would, of course, treat Logic as the theory of thought, or the study that is concerned with thought as a subjective activity, and would leave to epistemology the inquiry into the objective or cognitive validity of thought as knowledge. All that Wyclif can say is that actual knowledge involves unhesitating belief and the truth of the object. There is no proper definition of knowledge, no discussion of its nature, nothing, in fact, which does not make us feel that our epistemology of to-day, with all it has to say of the relation of subject and object in knowledge,

[1] *The Will to Believe*, p. 151.

belongs to a world that Wyclif never knew. The question of beginning and end is as troublesome to Wyclif as it has been to Logicians generally. We ought, according to Wyclif, rather to say that God *is*, than that He *was*, before the world, eternity being, with Him, anterior to the creative moment by nature, not in respect of time. Wyclif recalls for us the Lotzean conception of God, not as conceived *in* time, but as the Founder of Time. To Him, as raised above the succession of moments in virtue of His Eternal Absoluteness, the beginning and the end are one. Certainly we cannot make time the form of His life, but must rather make Deity the seat of Time. *To begin* means for Wyclif the first *now* of being and the last of non-being, while *to end* means the last *now* of being and the first of non-being. The conception of non-being, so introduced, is one which has proved very troublous to philosophic apprehension, as every one familiar with the history of the conception knows, and the last word has not been spoken upon the subject. It is right that Philosophy should inquire into the nature, reality, and necessity of non-being as the opposite, or rather the negative of being. This category of non-being has not failed of attention in recent philosophy, stirred to the inquiry under lead of the Logic of Hegel. The metaphysical tendency of Wyclif is again shown in his disposition to discuss such questions as the cessation of *non-entity* at creation, and the Lordship of Deity anterior to the Creation. To-day we are still positing creative energy in Deity that annuls non-being, and calls forth, as the Logos, creaturely existence out of this sphere of non-being. Wyclif also (p. 214) discusses the possibility of the Infinite being infinitely increased, and thinks we may come to know the Infinite by comparison with the Finite. We need not wonder that Wyclif seems haunted by the quantitative infinite, seeing it had received attention from Aristotle, and has shown such wonderful persistence even in modern Philosophy. We are concerned rather to remember that we could not even know the finite as such, but for its opposition to the Infinite. The Infinite is really prior, as positive condition of the finite, for how, save as first knowing the Infinite, could we really know the finite? So close in with the very possibilities of thought lies the Absolute, as the primary logical element.

In Dziewicki's second volume, we have Hypothetical Propositions discussed and classified. As the Hypothetical judgment is really an abstract universal, this discussion perhaps not unsuitably paves the way for Wyclif's taking up thereafter the problem of the Universals, when he wages war with the Nominalists. Of course, the thoroughgoing Nominalist makes the name the only general element—universals being to him mere words (the *flatus vocis* of Roscellinus)—but the mediating Conceptualist position imports the universality of the idea as well, and it means a return towards realism. The Conceptualist position at once affirms the particulars to be real, and the universal to be distinctly the pro-

duct of thought, language being, as expressive of knowledge, the bond that unites them. The Conceptualism of Abelard went so far with Nominalism as to hold nothing real but the individual, and even in the individual only that which really is individual. The conflict between Nominalism and Realism was indeed the basal one in Mediæval Philosophy, and the influence of Aristotle, though he was by no means a Nominalist, proved paramount in drawing off thought in a Nominalist direction, which so well harmonised with the interest of Science and exact knowledge of the concrete. For there can be no doubt that mediæval thinking showed a tendency to identify the real with that which was merely abstract or logically existent, so that at last a nominalistic type of thought is seen to prevail. A more moderate type of Realism than that of Wyclif found favour with most of the Schoolmen, and our Editor is careful to point out how much more difficult it is for the Nominalist to meet such a realistic attitude, which puts the Universality, as we know it, formally in the mind (*formaliter in mente*), and fundamentally in the objects (*fundamentaliter in re*). It does not seem desirable to follow all the windings of the extended discussion on Universals by this "Doctor Resolutissimus," as his Editor is disposed to style him. It gives interest to one's study of Wyclif's bold and powerful presentation, however, to remember how little this controversy over Universals is an outworn theme, how far indeed the question is from having reached any final solution in its scientific bearings to-day.[1] The form has become modernised, but the problems are the same, so soon as men take up the theory of knowledge, from which they are indeed inseparable. The Scientific Realism of to-day is speedily righted by a critical epistemology whensoever it fails to do justice to the idealistic elements in all knowledge. But science is not a thing of mere naming, nor its truth a thing of mere words, nor are its classifications necessarily arbitrary. We may grant to realism the truth of the types and classes, the genera and species, of science, without denying some wholesome force to the Nominalist contention that our conventional general propositions have need of the corrective influence of individual things. We admit the Nominalist contention as to the symbolic use of language, but we deny the Nominalist insistence on a particular image or a pictorial representation. The Conceptualist position is certainly to be preferred, whereby general terms and relations can be thought or conceived by mind, and general terms do represent ideas in the mind and qualities exterior to it. These terms do not thus, however, represent anterior forms of being as urged by realistic contention. The need and place for all these forms of thought—realistic, nominalistic, and conceptualist—were eventually disclosed when writers like Albertus Magnus showed how universals are *ante res* in the Divine or archetypal mind; are, at the same time, *in rebus* in respect of their common nature;

[1] *Cf.* Windelband, *History of Philosophy* (Eng. edit.), p. 229.

and are, likewise, *post res* as abstracted from things by the mind. But there is little room for wonder that men early felt the need to avoid any conceptualist mode of treating these universals as such unreal conceptions that the work of Science, in comparing and classifying and striving to reach general truths, should have been stifled. The ideal, the universal, must always be correlated with the actual, the particular, and their reciprocal character no more overlooked.[1] Wyclif found it hard enough to steer clear, in his Realism, of that pantheistic tendency which Abelard so often charged home upon Realism as its logical issue, and this may be said even if the pantheistic element amounted to no more than a tendency. This pantheistic danger comes very near to Wyclif in the use he makes of the conception of Transcendent Being, as something common to God and the creature, and he eludes the danger only by great logical adroitness and argumentative subtlety.

A discussion on Causal Propositions follows (chap. v.), in which Wyclif makes dexterous use of the Conception of modal, not substantial, difference, and holds that accidental modes in God are not to be viewed as identical with His Essence. Wyclif's thought here inevitably recalls the Hegelian identification of immanent power or force with its manifestation, its identification of the non-ego with the ego, but Wyclif would have been swift to perceive what a purely subjective process and what a purely notional result we have in all this movement, which is only that of mind itself. For all things are then real *in concreto* only in so far as they are moments in the vast unitary development which sums the dialectical necessity of the Hegelian mind. Wyclif maintains that no inherent accident has any existence *per se,* or adds anything to the substance (p. 103). But the notion of substance has its difficulties to this day. The meaning of substance cannot be widened, so as to be synonymous with being, Wyclif tells us (vol. i., p. 154), unless we are content to reduce accident to the sphere of non-being. But this widening is precisely what Spinoza later did not hesitate to do—to make everything substance, for that everything exists. Whether Spinoza meant it or no, what he succeeded in doing was to reduce all things to substance in the abstract, or the ultimate abstraction of *ens* or pure being. Not that he intended to designate by substance Being in general, but rather, in his severe renunciation of the determinate and particular, the absolute unity or complete totality of things. None the less does his mode of negativing the finite lead to infinite substance which is merely dead or indeterminate being. Hence the defect of Spinoza's view of the absolute substance, which is not as yet living and self-determining. Nothing is substance to Spinoza but what exists *in et per se,* the substantiality of an *ens causatum* or an *ens per aliud* being thereby denied. As if it were not enough that substance—even such real substances as we ourselves are—

[1] *Cf.* on this point De Sarlo, *Saggi di Filosofia*, vol. ii., pp. 140-148. Torino, 1897.

should be conceived *per se*, without needing to exist *in se!* So was it that mediæval Realism, with its Scholastic conceptions of the most real being, found in Spinoza its last term or final development.

At the close of the chapter, Wyclif remarks on the origin and possibility of sin in a somewhat unusual manner. For, though Wyclif locates defect in us, not in God, yet as First Cause of all, God has the possibility of our sinning fixed on Him as a real contingency (*summa contingentia*, p. 108), along with other indirect responsibilities. Wyclif's thought, like that of some thinkers still, seems to fail of realising the implications of our being free and finite agents in a moral system of things. It does so because due scope is not allowed to the free self-determination of man, the causative agency of God so haunting it. No one who thoroughly understands what rational free agency involves would set our peccability, our liability to sin, in such close relation to God's Absolute Causality as Wyclif does, but would relate it more to our own free choice or volition. The idea of Causal Efficiency has obscured for Wyclif the light that illuminates us when, as free though fallible creatures, we act through reason's laws and truths. It seems also hid from Wyclif's view, as it is from the thought of some thinkers to-day, how the action of God is really designed and directed, under a free moral system, towards the prevention of evil and sin, even though, in our imperfect knowledge of the Absolute Reason, we may sometimes be tempted to wonder why God does not do more.

The next chapter (vi.) suggests the tendency of Scholastic Philosophy to apply mathematical methods to metaphysical subjects—a tendency by no means confined to that Philosophy, however freely Wyclif may reveal it. A very curious result emerges in this chapter, when Wyclif seeks to rectify any wrong or unguarded impression which the previous chapter may have given touching God's relation to evil. God cannot, it seems, make man commit moral evil, but, the evil taking place, He can make such fact of sin to be good, for the sin is true and therefore, according to Wyclif, good. A rather specious interpretation, it must be said, when the result or inference, as existing good, is credited to Deity, to whom, however, the premiss is not attached. Whatever exists must be good, according to Wyclif, so that evil practically ceases to be evil and has become good—a position which we have found paralleled in our own generation, but happily not often.

We pass to notice the third of Dziewicki's volumes. Here we are again impressed with the inutility of many of the Scholastic subtleties, but who shall say that many problems which to-day agitate the Philosophical mind may not, as many centuries after, appear quite as useless or absurd? Even these mediævalisms have, for us, a certain historic interest, and are much more than simply intricate puzzles of the past. Omitting much that is

curious in Wyclif's theory of time and space, we pause to notice his conception of the Infinite. God alone is infinite (p. 36), but whatever exceeds our conception is infinite even when not infinite in itself. What may be infinite to one will be but finite to another, and in neither case will be infinite to Deity. All which is, at least, as intelligible as the modern postulation at once of finitude and infinity for Nature, where the infinity does not mean "more than a given amount of existence".[1] The success of attempts, whether mediæval or modern, to apply the laws of logic to the Infinite may not always have been impressive, but we nevertheless welcome and give them free course. Meanwhile we are not content to conceive the Infinite in any such derived and secondary fashion, as would be implied in making it the mere negation of the finite.

An interesting point of difference from Scholastic theory meets us later in Wyclif. He does not simply follow the ordinary Scholastic dualism as to form and matter. Matter and form are not to him absolute separable realities; he postulates a sort of trinity (*modus trinitatis*, p. 121), consisting of matter, form, and compound, in which all are different though in a sense identical. Matter and form are to be regarded qualitatively rather than as quantitative parts, and matter is, in Wyclif's view, eternal (pp. 121-122). It is, of course, quite feasible to conceive matter as eternal, and yet hold creation to be necessary in order to give it form. The Aristotelian distinction could in the Scholastic period be retained, and yet room found for creation in an absolute sense, for those who chose so to regard the issues, however repugnant the notion of such absolute origination might have been to Aristotle himself.

In the next chapter (x.) Wyclif sets out by denying that whatever is known is where the knower is, on the ground that everything would thus be everywhere with God. Wyclif works his way, through varying degrees of interest, up to a lengthened discussion of Time. Time is eternal as the World (p. 162), and Time is everywhere (p. 163). *Is*, with him, really extends to all time, and signifies, in fact, eternity (pp. 174-177). Time requires a *before* and an *after*, and these are found only in movement, without which there is no time (p. 211). If only imagination moved, this would itself be movement (p. 213).

Looking back over the whole three volumes of this work, it remains to be said that, if we have not cared to indulge in peddling criticism, that is both because we have judged it more important broadly to indicate the comprehensive and subtle character of Wyclif's mind and his dexterous skill in handling the weapons of Logic, and because we are too grateful for the wide learning, great acumen, and patient skill which his Editor,

[1] *Appearance and Reality*, ch. xxii.

M. H. Dziewicki, has brought to bear upon his toilsome task. Discount his work as any may, to him it may still, with as much cordiality as truthfulness, be said—*Exegisti monumentum.*

JAMES LINDSAY.

The Origin and Growth of Plato's Logic. With an account of Plato's style and of the chronology of his writings. By WINCENTY LUTOSLAWSKI. Longmans, Green & Co., 1897. Pp. x., 547.

THIS is an interesting and important work by an enthusiastic student of Plato. The substance of a considerable portion of it has already appeared in a condensed form in the *Archiv für Geschichte der Philosophie*, vol. ix., pp. 67-114, and elsewhere; but the present volume is the first full and complete account of the author's minute and laborious observations.

In an introduction of sixty-three pages Mr. Lutoslawski reviews the theories of his predecessors on Plato as a logician, and on the authenticity and chronology of Plato's writings. The result is to show that a large number of investigators, working to some extent on independent lines, are beginning to agree in maintaining the late origin of the so-called dialectical dialogues. This conclusion Mr. Lutoslawski attempts to expand and justify by arguments drawn (1) from the style of Plato, (2) from a comparison of the logical contents of the different dialogues. Accordingly, the first part of the work (pp. 64-193) is, primarily speaking, philological, the second (pp. 193-527) philosophical.

The philological section, though ostensibly only preparatory to the exposition of Plato's doctrines in part ii., is in our judgment the most important and original portion of the whole work. Mr. Lutoslawski presents us for the first time with a complete account of the stylometric observations which were begun by Campbell, and have been continued by a large number of students in Germany and elsewhere. He attempts to sift and classify them, determining which are significant for the student of Plato's chronology, and which should be neglected. A list of 500 significant peculiarities is drawn up and employed to determine the "stylistic affinities" of the different dialogues by a new and original method. It is assumed—and no one will quarrel with the assumption—that Plato's latest dialogue is the *Laws*, and accordingly "The Laws are our standard of comparison for the next latest five dialogues, and for earlier works the group of the six latest dialogues, *Sophist*, *Politicus*, *Philebus*, *Timæus*, *Critias*, *Laws*" (p. 153). The number of stylistic peculiarities occurring in the various dialogues determines (within certain limits) their date of composition, allowance, of course, being made for their relative size. But it is also necessary to take account of the

importance of the peculiarities observed. This Mr. Lutoslawski does by arranging the peculiarities in four classes, very important, important, repeated, and accidental, and counting the first as equivalent to four, the second to three, the third to two, accidental peculiarities. We thus obtain a large number of "units of stylistic affinity" and the relation between different dialogues can be estimated to a decimal point.

Such, in outline, is the method of the "Science of Stylometry," as laid down by Mr. Lutoslawski. The author regards it as a "new science of style, which will enable us to decide questions of authenticity and chronology of literary works with the same certainty as palæographers now know the age and authenticity of manuscripts" (p. 193). Truly a glorious prospect! The birth of such a science awakens high hopes; but will it survive the ἀμφιδρόμια of criticism? *μετὰ δὲ τόν τόκον τὰ ἀμφιδρόμια αὐτοῦ ὡς ἀληθῶς ἐν κύκλῳ περιθρεκτέον τῷ λόγῳ, σκοπουμένους μὴ λάθῃ ἡμᾶς οὐκ ἄξιον ὂν τροφῆς τὸ γιγνόμενον, ἀλλ' ἀνεμιαῖόν τε καὶ ψεῦδος* (*Theæt.*, 160 E).

Unless we are mistaken, many scholars will be reluctant to allow that the true essence of style is capable of being "weighed and measured and counted" in this fashion. *τί δαί; ταλάντῳ μουσικὴ σταθμήσεται;* will occur to not a few. Others will admit that such a process is legitimate enough in itself, but will deny that "stylistic affinity" of this kind affords a sure and inevitable basis on which to build a chronological theory of Plato's dialogues. Surely an author *may*, if he chooses, *deliberately* imitate his own earlier or later style, for some special purpose. And the possibility of such intentional stylistic variation is least of all excluded in a writer of Plato's infinite variety and scope. A still larger number of scholars will, in our judgment, be unwilling to allow that Mr. Lutoslawski's peculiarities are sufficiently important in themselves to justify any certain conclusion as to the relative order of the dialogues. Some of them are highly noteworthy, and their cumulative force is considerable; but a large number appear to be extraordinarily trivial. On page 102, for example, the varieties in Plato's use of the dual are made to furnish no less than fifteen stylistic peculiarities, and among them the following: *ταῖν* (*Polit.* 1, *Tim.* 1, *Legg.* 3), *νῷν* (*Symp.* 1, *Phædr.* 1, *Theæt.* 1, *Soph.* 1, *Polit.* 3, *Phil.* 4, *Legg.* 2), *τέχνα* as dual of *τέχνη* (*Rep.* 2, *Soph.* 1, *Polit.* 1, *Legg.* 1), *σφῷν* (*Euthyd.* 4, *Theæt.* 1, *Legg.* 12). The occurrence of these particular forms is surely no evidence of date, unless we can prove that the exact meanings which they bear could have been, and often were, otherwise expressed by Plato in dialogues of a different period. In point of fact, Mr. Lutoslawski himself is occasionally driven to adopt a different and far more natural solution. If *σφῷν* is a 'peculiarity of later style,' how comes it that Plato uses the word no less than four times in a work so early as the *Euthydemus*? Mr. Lutoslawski has his answer ready: "This coincidence between *Euthyd.* and *Legg.* Roeper

explains by the circumstance that in both dialogues one person is speaking to two others, intimately associated" (p. 102). Such an explanation is surely satisfactory, and the principle on which it rests is one of wide, and indeed universal, application. The Platonic dialogues differ not only in scene, setting, and dramatic situation, but also in subject and treatment; and a particular 'stylistic peculiarity' is often nothing more than the inevitable expression of the peculiar shade of meaning which the author wishes to convey. And we have no reason to suppose that a particular shade of meaning indicates in *every* case a particular date or period in an author's literary activity.

The general verdict on Mr. Lutoslawski's stylometric investigations will, we think, be "Not proven". Without denying the provisional validity of his fundamental principle, we desiderate a still more exhaustive enumeration of stylistic peculiarities, and a much more careful and scholarly sifting of those already enumerated. In particular, many of the arguments at present based on community of vocabulary should, in our opinion, be discarded. We cannot, for example, assign any value whatever to the following, among several other instances of this kind: ἰστίον (*Parm.* 1, *Legg.* 1); ἀπέχω = disto (*Rep.* 1, *Parm.* 2, *Tim.* 1, *Crit.* 2, *Legg.* 2), πᾶν ζῷον for 'every animal' (*Rep.* 1, *Polit.* 1, *Phil.* 2, *Tim.* 2, *Legg.* 5). The observation of linguistic peculiarities in Plato has been too often conducted in a mechanical spirit, without sufficiently distinguishing between the various connotations of words and particles. It may be hoped that the new Platonic lexicon, under the editorship of Professor Campbell, will do something to remedy this defect.

In the second part of his book Mr. Lutoslawski traces the development of Platonic doctrine from the *Euthyphro* to the *Laws*, frequently supplementing the chronological results of part i. by arguments derived from the treatment and subject-matter of the dialogues, and from cross-references, allusions to contemporaries and the like.

Three leading periods are distinguished: (1) the Socratic, comprising the dialogues generally admitted to be early, down to the *Gorgias*, which forms the transition to the second stage; (2) the period of middle Platonism, introduced by the *Cratylus*, *Symposium* and *Phædo*, and including the *Republic* and the *Phædrus;* (3) the later period, to which belong the two introductory and critical dialogues, *Theætetus* and *Parmenides;* the *Sophist*, *Politicus*, and *Philebus*, containing Plato's new theory of science; and finally the three dialogues which represent the latest development of Plato's philosophy, *Timæus*, *Critias* and *Laws.* The gradual growth and development of Plato's philosophy throughout this sequence of dialogues is skilfully traced, although occasionally the author seems to lay undue stress upon trivial or even wholly insignificant points. Few will attach importance to the argument that the *Symposium* is later than the

Protagoras, because "the rule laid down in the *Protagoras* (347 C) to exclude flute girls and similar artists from philosophical banquets is repeated in the *Symposium*" (176 E) as if it were "a matter of course, while it is explained at length in the *Protagoras*" (p. 243). Still less need we suppose that the *Parmenides* is late, because what is there "repeatedly said of youth (130 E, 135 D, 137 B), that young men are inconsequent, that one must learn while young, and that youth is pleasing and compliant, is only explainable if the author was, comparatively speaking, an old man when writing" (p. 408). Why so? Such remarks are surely sufficiently explained by the age of Parmenides, in whose mouth they are placed. Why need we suppose that every one who paints old age is old himself? Other questionable or erroneous observations will be found on pp. 314, 315, 355. The last of these passages deserves to be quoted as a characteristic specimen of the arbitrary identifications which are not uncommon in investigations of this kind. "If Plato in the *Phædrus* credits Simmias with a greater merit, this means that he preferred his *Phædo* to the *Symposium*." Here Mr. Lutoslawski appears to us in danger of forgetting that the spokesmen in Plato's dialogues are not characters in a novel, but historical personalities. Why should we interpret the man Simmias as the dialogue *Phædo*, or the man Phædrus as the *Symposium* of Plato?

The most interesting and important point in Mr. Lutoslawski's exposition of Platonic doctrine is connected with the theory of Ideas. In the later Platonism the separate existence of ideas, according to him, is entirely abandoned. "Ideas exist only in souls—they are eternal and unchangeable because their first model is created by God in his own thought" (p. 523). Even in the earlier period, we are told that "the separate existence of ideas outside any mind has never been expressly affirmed in clear words" by Plato, "because the poetical metaphors of the *Phædrus*, *Republic*, *Phædo* and *Symposium* cannot be taken as literal expressions of abstract truth" (p. 447). Mr. Lutoslawski here touches on the fundamental difficulty of Platonic criticism, *viz.*, what is the proper place and function of the allegorical method of interpreting Plato. It is perhaps too much to expect that scholars will ever agree on this question. One man will always see an allegory in that which to another seems only a statement of fact. The appeal in the last resort must always be to Plato's language, and, tested by this standard, the theory represented by Mr. Lutoslawski appears to us untenable. The expressions *αὐτὸ καθ' αὑτό* and even *αὐτό* alone, which are constantly used of the Ideas in Middle Platonism, in our judgment imply the self-existence of the Ideas. *αὐτό* means 'by itself,' exactly as in the idiomatic *αὐτοὶ γάρ ἐσμεν*. If Aristotle had never written a syllable about Plato, the *Phædrus* alone would have been enough to give currency to the traditional belief in the transcendence of the Ideas. The adhesion of Aristotle, the whole of whose criticism of Plato loses its force

on any other supposition, might have been expected to remove the subject from the sphere of controversy. But Allegory, like Proteus, escapes from every fetter; and to Mr. Lutoslawski, even the "pure ideas" of the *Phædrus*, "without shape or colour, intangible and invisible, not fixed in sensible particulars, but free and independent," signify only "that pure concepts of reason are never fully realised in the things to which they apply, as for instance, absolute equality is never found identical with physical equality" (p. 340). With regard to later Platonism, Mr. Lutoslawski is still more emphatic. Nothing will induce him to admit that there is a single passage in works written after the *Parmenides* "supporting the assumption that ideas exist outside every soul" (p. 448). The well-known passage in the *Timæus* (51 E-52 A) is therefore, we suppose, "allegorical".

Enough has been said to indicate the general character of this work. Although we are unable to agree with much of the reasoning, we do not deny that the conclusions as to the order of the dialogues may be, generally speaking, sound. In any case there can be no question that Mr. Lutoslawski has rendered a great service to Platonic scholarship. Of his industry, zeal, and enthusiasm, it would be impossible to speak too highly; and his acquaintance with the literature of his subject is probably unique. His book is one of the most suggestive and stimulating works on Plato which the present generation has seen.

J. ADAM.

La Philosophie de Charles Secrétan. Par F. PILLON. Paris: Félix Alcan. Pp. 197.

OF the three chapters of which this work consists the first two contain an admirably clear account of the fundamental ideas in Secrétan's metaphysics and ethics respectively. The third, which takes up rather more than half of the book, contains M. Pillon's historical and critical estimate of Secrétan's philosophy. In the earlier sections of this chapter the author first traces some changes which Secrétan's views underwent in his later works, especially in regard to the influence which the conception of evolution had upon his thought; and then proceeds to compare Secrétan with certain other thinkers—Plotinus, in whose system M. Pillon finds an interesting parallel to Secrétan's conception of absolute liberty, Duns Scotus, and finally Descartes, whose points of difference from and contact with Secrétan are discussed at some length. M. Pillon then enters upon his own criticism of Secrétan's philosophy, dealing successively with Secrétan's conception of liberty, his theory of creation, which is found to be pantheistic, his conception of a divine eternity, his professed conciliation of monism and monadology, and finally the fundamental doctrines of his ethical theory. It is to M. Pillon's criticisms, naturally, that the reader turns with most interest, and it is on them that I have to offer some critical remarks.

That Secrétan's conception of absolute liberty should be severely criticised was to be expected, but I think that M. Pillon in his justly severe criticism of the conception hardly gives the recognition they deserve to the speculative motives which underlie it. Secrétan argues, in effect, that God, if he is to be truly absolute and unconditioned, cannot be limited by any laws given to him from without. Therefore we cannot say simply that God acts according to his nature, for then his nature would be a limit imposed upon his action. To give full effect to the absolute self-determination of God we must say that God gives himself the laws of his own nature, he makes himself what he is, he is what he wills to be, his liberty is absolute. As against this argument as it stands M. Pillon's answer (which occurs in another context, but is quite general in its import) is conclusive: "Si la liberté consiste à se faire ce qu'on est, à être l'auteur de son être, de sa nature, il n'y a pas de liberté; car pour déterminer ou faire quoi que ce soit, il faut d'abord être quelque chose" (p. 108). In other words, the conception which Secrétan requires us to think is a paradox. It is impossible to think that which gives itself the laws of its own nature, *before* it has given itself these laws—and this is equally true whether the antecedence be logical or temporal. M. Pillon is therefore right when he says that logic will not allow us to admit Secrétan's conception of liberty. At the same time it is only fair to recognise that this criticism does not get rid of the standing metaphysical problem which Secrétan's conception expresses in so striking a way. If we cannot accept Secrétan's way of securing the absolute unity of God, neither can we acquiesce, from the absolute point of view, in the dualism of action according to a nature already defined. M. Pillon elsewhere charges Secrétan with employing in his so-called 'liberty' a psychological term which, in his metaphysical application of it, is really emptied, though Secrétan does not seem to see this, of all its psychological meaning. And the criticism, no doubt, is for the most part just. M. Pillon insists that the term 'liberty' must be used in its proper, *i.e.*, its limited and relative sense. "Ce qui caractérise," he says, in his discussion of Secrétan's view of evolution, where the criticism has a special appropriateness, "Ce qui caractérise et définit l'acte libre, et nous ne voyons pas comment il pourrait être autrement défini, c'est qu'il ne dépend qu'*en partie* de la nature de l'agent. Il est vrai, d'autre part, que l'être libre se fait soi-même; mais cette proposition doit aussi être restreinte: l'être libre se fait soi-même *en partie*" (p. 109). But obviously, if we criticise Secrétan entirely from this relative standpoint, we are virtually excluding from our view the real speculative problem. And I think that M. Pillon has hardly recognised this, or at any rate has omitted to express himself on the subject in the present work.

I find a very similar omission in his criticism of Secrétan's views on the question of the divine eternity, but as the case is similar in principle I need not stay to consider it.

In his criticism of Secrétan's attempted conciliation of monism and monadology M. Pillon objects to Secrétan's treatment of psychological individuality, the individuality of self-consciousness. Secrétan has argued that when we consider the contents of self-consciousness, and not merely its form, the isolation of the individual becomes impossible, the apparently individual mind is found to be involved in countless ways with other minds, so that its very existence depends upon its sharing a common life. Accordingly we must regard mere self-consciousness as only a form or limit. And in Secrétan's view it is only in moral individuality that we find a real barrier to monism. M. Pillon contends that Secrétan's depreciation of the individuality of self-consciousness depends upon his Kantian and subjective estimation of the forms of thought, and that, so far as moral individuality operates by means of these very forms, it also is formal and unreal. As against the substance of Secrétan's argument this criticism seems to me rather far-fetched and ineffective. Whether or not one accepts Secrétan's doctrine of the merely formal character of self-consciousness, one may surely admit the broad truth of the view that it is pre-eminently in action that the individual recognises and realises himself as such.

In dealing with Secrétan's ethical theory M. Pillon very rightly begins by rejecting altogether the illusory metaphysical basis upon which Secrétan thought to construct it—the identity in point of substance of all finite existences with each other and with God. "Fût-elle une réalité métaphysique," says M. Pillon, "l'identité de substance serait absolument étrangère à la morale" (p. 179). But the two criticisms by which he proceeds to justify this rejection of Secrétan's metaphysical basis of ethics seem to me somewhat open to question. M. Pillon appears to argue that Secrétan's error lay in basing moral duties upon merely factual or metaphysical relationships, whereas the relationships in question are really moral, and are themselves *based upon* moral duties, while moral duties cannot be based upon anything except the ultimate notion of duty itself. "Ce n'est pas la charité qui dérive du rapport de la partie au tout, de l'organe à l'organisme; c'est, au contraire, par la charité que s'établit ce rapport" (p. 182). I think that M. Pillon is here tending towards an over-statement of the real objection to Secrétan's doctrine. Secrétan's error lies not so much in assuming the existence of the relationship, as in defining the relationship in terms which have no moral content, no bearing upon the moral end. As M. Pillon rightly says, Secrétan might as well have based his ethics upon a physical or a chemical unity as a metaphysical one like that of substance—for they are all alike in having no moral significance whatever. On the other hand, it is clear that there must necessarily exist between the individual and those towards whom he has duties, spiritual or moral relationships, which are the specific ground of these duties; he must be able to contemplate those towards whom he has duties as connected somehow with the moral purpose of his life, otherwise it is impossible

to see how they could be anything to him or he could have any duties to them. "Vous devez vous unir de volonté aux esprits créés, vos semblables," says M. Pillon (p. 182). One might be hypercritical enough to ask whether the fact that we belong to the same species of created beings is any more a ground of obligation than our participation in the same substance. But here we touch M. Pillon's second criticism of Secrétan's doctrine. Apparently M. Pillon would altogether refuse to look for any such ground of obligation as that here assumed to exist. He objects that, in making identity of substance the moral motive (or, as I should prefer to say, the basis of obligation), Secrétan is virtually resolving 'la charité' into 'un amour-propre éclairé, réfléchi, approfondi': because the individual only seeks the good of others in so far as it is also his own good, the good of the whole of which he is a part. I confess I cannot agree with the criticism, but it would of course be out of place to enter upon this controverted topic here.

There is one other point that seems to call for remark in M. Pillon's criticism of Secrétan's ethical theory. Secrétan has argued that his fundamental ethical principle, 'charity,' contains within itself, and is in fact the true foundation of, the principle of justice, *i.e.* regard for the individuality or freedom of others; for 'charity' wills the good of the other, that is to say, it wills that he should realise his true nature, that he should will the good, and this means, since he is a free agent, that he should realise himself freely, that he should freely will the good. M. Pillon replies by distinguishing three senses of freedom: the metaphysical, the juridical, and the moral; and argues that, in strictness, 'charity' seeks only the moral freedom of others, and if left to itself would even at times use constraint to bring them to this moral freedom; consequently the principle of 'charity' must be supplemented and limited by an independent principle of justice. M. Pillon anticipates, indeed, the objection that juridical freedom is a condition of the development of moral freedom, but, just because he remains at the juridical point of view, he seems to me to miss the full meaning of the position which he is criticising. For so far as moral freedom implies an intelligent choice of the good for its own sake, it can be influenced only by moral motives. If therefore constraint can operate to produce moral freedom at all, it must operate by producing such motives. But if it operates in this way can we any longer say with perfect generality that it violates a right, or must we not leave that to be decided by a consideration of the conditions under which it is employed? When M. Pillon says: "le droit a besoin d'être protégé contre la charité, parce que la charité ne saurait trouver dans son principe ni dans son expérience l'obligation de respecter le droit" (p. 197), he is surely going too far.

But, although one may not always agree entirely with M. Pillon's judgments, there can be no question that his work is an excellent piece of exposition and criticism.

H. Barker.

VII.—NEW BOOKS.

Studies in the Psychology of Sex. Vol. i.: *Sexual Inversion.* By Havelock Ellis. London and Watford: University Press, 1897. Pp. xvii., 204.

This book (which has already appeared in German in a translation by Dr. Kurella) is the first volume of a work in which the author proposes to deal, from a general psychological rather than merely medical standpoint, with some of the main normal and abnormal aspects of the sexual impulse. The work will probably extend to five or six volumes, and (as explained in the special preface to this first volume), though it would be more logical to begin with the study of the normal sexual instinct, the present volume, in consequence of the assistance rendered by collaborators, was completed first.

In a "General Preface" to the whole work, the author states that he has had this task in view for more than twenty years, and that a psychological and anthropological study of human secondary sexual characters (*Man and Woman*), published some years previously, was written as an introduction to it. The author also states his belief that "the question of sex—with the racial questions that rest on it—stands before the coming generations as the chief problem for solution. Sex lies at the root of life, and we can never learn to reverence life until we know how to understand sex."

In the "Preface to Volume I.," the author points out the special interest attaching to the problem of sexual inversion, and remarks that before his first cases were published, "not a single British case, unconnected with the asylum or the prison, had ever been recorded". He adds: "Very few indeed would not be surprised if it were possible to publish a list of names of sexually inverted men and women who at the present time are honourably known in Church, State, Society, Art or Letters".

Congenital sexual inversion, defined as "sexual instinct turned by inborn constitutional abnormality towards persons of the same sex," is the chief subject of the book. The first chapter ("Introduction") is, however, mainly concerned with a brief general consideration of the undifferentiated phenomena of homosexuality among animals, the lower human races, soldiers and the lower social classes in Europe, criminals, and men of exceptional intellectual or moral aptitude (Michelangelo, Winckelmann, Whitman, Verlaine, etc.). The theories which regard climate and race as the sole factors of homosexuality are stated, only to be dismissed on account of the universality of the phenomena.

The second chapter reviews the scientific study of sexual inversion from Westphal (1870) through Ulrichs, Tarnowsky, Krafft-Ebing, Moll, Raffalovich, etc., to the present. "As it now presents itself, it is a psychological and medico-legal problem so full of interest that we need not fear to face it, so full of grave social actuality that we are bound to face it."

The next chapter, dealing with inversion in men, begins by touching

upon homosexual phenomena in schools, and discusses whether, as Max Dessoir states, the sexual impulse is undifferentiated in youth, so that (as Prof. James believes) every one has the "germinal possibility" of inversion. Only a qualified agreement is given to this conclusion. The sexual impulse in youth is *relatively* undifferentiated. Complex classifications of inversion are rejected, only two classes being accepted: (1) simple inversion, and (2) psychosexual hermaphroditism (*i.e.*, with an attraction to both sexes); in both classes the inversion may be either congenital or acquired. Really acquired inversion (*i.e.*, without any congenital basis of abnormality) the author believes to be extremely rare. The histories of twenty-seven new cases are presented in this chapter.

In the following chapter inversion in women is fully dealt with, and four new cases published.

In chapter v. the author analyses his own cases, thirty-six in number (including five that remain unpublished). As regards race, the cases were with three exceptions (Americans) all British; in four there was a German element, possibly a significant fact. Twelve of the subjects belonged to ordinarily healthy families. In the others there was disease or abnormality, usually of a mild character (eccentricity, neurasthenia, alcoholism, etc.). The personal health was in twenty-six cases good, though some of these possessed a highly nervous temperament; eight were in delicate health, one in very bad health, and one became insane. In twenty-nine cases the abnormal instinct appeared early in life without any previous attraction to the opposite sex; in only four cases was there any change of impulse towards the same sex in adult life; in all these latter four the sexual instinct, or at all events the general health, was weak. In nineteen cases the attraction was marked before puberty, in such cases its manifestations being usually psychic. But the precocity of the abnormal instinct remains marked, and this fact is probably significant. In about one third of the cases there is reason to believe that some event, or special environment, in early life had influence in turning the sexual instinct into homosexual channels, or calling out a latent inversion; but the author gives grounds for believing that the influence of suggestion has by some writers been greatly overestimated, and that suggestion is inoperative when no predisposition exists. Attraction to both sexes is much rarer than simple inversion, only five of the author's cases being thus attracted. It is pointed out that even in inversion a kind of pseudo-sexual attraction rules, the invert being attracted to a person unlike himself or herself, though of the same sex. Inverts are not usually attracted to one another. The male invert is by no means always feminine in disposition and appearance; but at the same time there is a general tendency, often not obvious, to approach the feminine type. The author suggests that there is also a tendency among inverted persons to be somewhat arrested in development and to approach the child-type, and associates this tendency with their sexual precocity. Sexual manifestations vary very greatly in different individuals; in three cases, from moral and other reasons, there had been no physical manifestations. Artistic aptitude was found in no less than twenty-four of the cases, or 66 per cent. as against 30 per cent. (according to Galton) for the general population; a marked taste for physical science was found in no case, though three of the subjects were medical men. The moral attitude was noted in twenty-nine cases; three loathe themselves; six or seven are doubtful; the remainder, a large majority (including all the women), consider their moral position the same as that of normal persons, while one or two even regard inverted love as nobler than ordinary sexual love, provided there is mutual understanding; the chief regret in a few cases is that they are obliged to lead a double life.

The following chapter discusses and criticises the various theories put forward in explanation of sexual inversion. The author compares inversion to colour-blindness, coloured hearing, and similar abnormalities arising on a hereditary basis, and points out that abnormality is not necessarily disease. Embryonic hermaphroditism is regarded as the key to inversion. In emphasising the importance of the congenital element in inversion, the author points out that no hereditary transmission of emotions and ideas is involved, but simply the transmission of a more or less modified reflex mechanism. On this point the author is in full agreement with the very elaborate discussion in Moll's more recently published *Untersuchungen über die Libido Sexualis*.

The last chapter is devoted to "Conclusions". For the prevention of spurious homosexuality the author advocates co-education and the healthy mingling of the sexes from childhood onwards. With regard to the medical treatment of this condition, the author chiefly advises removal of the associated neurasthenia, etc., and severely condemns the radical method of Schrenck-Notzing (frequent hypnotic sittings combined with visits to a brothel) as worse than the disease. Even when successful, this "cure" merely succeeds in rendering the invert capable of experiencing both normal and abnormal impulses, and of procreating. Neither of these is a very desirable end. "Sometimes, indeed, the tendency to sexual inversion in eccentric and neurotic families seems merely to be Nature's merciful method of winding up a concern which, from her point of view, has ceased to be profitable." With regard to the legal relationships of sexual inversion, the author approves of the general tendency shown by new European codes not to regard homosexual practices as *per se* a criminal offence. "It should be the function of the law in this matter to prevent violence, to protect the young, and to preserve order and decency. Whatever laws are laid down beyond this must be left to the individuals themselves, to the moralist, and to social opinion."

The volume includes numerous appendices by various hands: a paper on "Homosexuality among Tramps," from personal observation, by "Josiah Flynt," a summary of "Ulrichs's Views," a letter in defence of homosexuality from an eminent American professor, the history of the Countess Sarolta V., and "A Note on Sexual Inversion" by an American woman physician.

H. E.

The New Psychology. By E. W. Scripture. The Cont. Sci. Series. London: Walter Scott, 1897. Pp. xxiv., 500. 124 Illustrations.

This work is a popular statement of the purposes of the New Psychology. It is written, the Preface tells us, to make clear the aims and methods of psychology, rather than to give a systematic exposition of its results or to cover the whole field of investigation. This intention is reflected in the main divisions of the work (Methods, Time, Energy, Space, Past and Present), which may be said to constitute an introduction with statement of thesis, three illustrative parts, and a history of the science. When we examine the subject-matter of the three illustrative parts, however, we see that much more is attempted than the modest statement of the Preface implies, and that they really contain a summary of results in most of the branches of experimental psychology.

The classification of the material of so large a field under three heads gives rise to some peculiar groupings. Under Time, we have, first, a

chapter on Standards of Time, that defines or describes 'time' and explains several instruments devised for its accurate measurement; then six chapters on Reaction Times; and finally a chapter on memory, called Time Influence, and one on association, disguised under the title of Succession in Time. Under Energy we find a chapter each devoted to Standards of Energy, Voluntary Action, Fatigue, Passive and Active Movements, Resistance and Heaviness, Lifted Weights, Pressure, Pain, Feelings, Sound and Colour. An obvious criticism is that the division is based on physical categories that have no counterpart in psychology, and that even the physical accompaniments of mental process do not always fall under the head in which the process is treated. Hearing and Sight are not chiefly distinguished by the amount of energy expended upon the sense organs in producing them; and it is more than questionable whether the temporal aspect of association is its most important one.

Examination of the single chapters shows a number of cases in which topics are treated in unusual proportions, or with emphasis upon what are usually regarded as minor points. Thus in the chapter on Active and Passive Movements we find 5 pp. devoted to a statement of the results obtained by Goldscheider and Müller and Schumann, and 9 pp. to experiments on table-tipping, telepathy and kindred phenomena of automatic movement. So, too, in the chapter on Lifted Weights: Weber's law, the method of r. and w. cases, and special results in this particular field, are allowed 5 pp., because "with Weber and Fechner they played an extremely important part in establishing the new psychology," while the influence of size upon apparent weight receives 10 pp., and is treated in great detail with reference to the age of the subjects experimented on, the extent and kind of knowledge of size, and the sense that gave rise to this knowledge. Some of this disparity in length of treatment is due, of course, to the popular interest of certain problems which are comparatively unimportant for the development of the science at large; but one is forced to the conclusion that much of it owes its origin to Dr. Scripture's personal interest. The effect of size upon apparent weight has been investigated in the Yale laboratory; the other work has merely a historical interest. A similar bias seems to be at work in the allotment of space in the chapter on association, perhaps to have led to the treatment of association at all in this connexion. Certainly no other psychologist would assign association in general less than 10 pp., and then use seven of them for the discussion of mediate association! The author has overlooked the difference between the existence of mediate association, which is fairly well proven, and the adequacy of his experimental method for demonstrating it, which seems to be pretty thoroughly disproven. Again, nearly all the apparatus described is of the author's own devising, and the appliances of his laboratory are noticed in detail. Such definiteness of setting may, in itself, give the reader a clearer conception of the methods of a science than could be attained by more general treatment; but it also supports the conclusion that the writer has been influenced in his choice of material by personal feeling. The footnotes show the same tendency. More than a quarter of them either refer to Dr. Scripture's own writings, or to the Yale Studies. True, the Preface says that the book is not intended to be a complete treatise, but rather to indicate the problems and methods of psychology. But the introduction of technical details in descriptions of apparatus and discussions of method, and the statement that certain tables and appendices are for the convenience of those who desire to use the book in the laboratory, give evidence that the author had other ends more or less clearly in view when he compiled the work.

More serious are certain errors, and an inadequate or misleading treat-

ment of different subjects, mainly in the optical portions of the book. In the chapter on Colour the Young-Helmholtz theory is the only one mentioned; it is assumed, in the explanation of colour mixture, as if it were an established fact. It is natural that the theory should be used where the chief emphasis is upon the laws of colour mixture, which it was invented to explain, and which alone of optical phenomena it can satisfactorily account for. But it seems undesirable, even in a popular work, to give the impression that one of several rival theories is the only one, and that it is entirely adequate to the facts. Listing's law is omitted in the discussion of eye movements, unless it be confused with the false torsion of projection on a plane surface; and several statements of the form of the horopter are either incorrect or very obscure. Hardly less serious is the new use of terms that have already a definite place in psychological terminology. Thus 'field of regard' and 'point of regard,' the recognised English equivalents of *Blickfeld* and *Blickpunkt*, are used in Wundt's metaphorical sense for field and point of attention, and even in this use are restricted in their application to the one sense of sight. Similarly, in the attempt to avoid technical expressions for the higher processes that are not to be discussed in the book, we find such words as will, attention, feeling and imagination employed with different meanings, where accuracy and accepted custom would require more definite phrases. All retrograde steps of this kind are particularly regrettable at the present stage of development of psychological nomenclature.

There is, however, much to be praised in the book. Its whole attitude is original. We are given a psychology written entirely from the subjective point of view, with very little reference to physiological or physical facts. There is a consistent attempt to describe and classify the phenomena of consciousness as immediately given, without regard to their conditions in past experience or anatomical structure. Whatever we may think of this as an ultimate method, we must at least agree that the time has come to take a survey of the results of psychology purely as psychology; and this the author does, in clear and concise form. A statement is made of the methods of experimentation, of the results, and of the variation of results under different conditions; and nothing more is attempted in the way of explanation. Sometimes, indeed, this negative attitude is replaced by a dogmatic assertion that we cannot and ought not to try to know more. As regards the fact of single vision with two eyes, *e.g.*, we read: "There is no question of why to be asked, any more than why red and yellow make orange. It is a matter of fact in either case, and psychology is concerned with determining the laws that govern such combinations." This sentence expresses the fundamental fault that we have been criticising throughout in its special manifestations. There is no sense of the larger whole, of a science which shall combine the discrete facts. Each problem stands by itself, and is of interest for itself alone. And while each problem is attacked with great ingenuity, there is no recognition of the need of proportion in treatment, no subordination of the separate parts to a single conception, of the particular investigations to a general investigation.

There is still some suggestion of a laboured writing down to the level of the ordinary intelligence, which has led at times to such sentences as: "The purpose of rhythm is to aid in clearness of the feeling for time intervals". On the whole, however, the reader has little to complain of on this score. The material is clearly presented, without falling to the juvenile story-book style. The illustrations are apt and simple, without being undignified. One is particularly impressed in this connexion by the striking use that is made of all the most recent inventions of the practical world. Tricolour photography, the kinetoscope and the polaris-

ing stereopticon are all made to contribute their share to the work. On the whole, it is one that has much value for the psychologist, and one that will undoubtedly receive a full meed of popular approval.

W. B. PILLSBURY.

The Psychology of Suggestion. By B. SIDIS. With an Introduction by W. JAMES. New York: D. Appleton & Co., 1898. Pp. x., 386.

This book, which bears the secondary title "A research into the subconscious nature of man and society," falls into three parts, dealing respectively with suggestibility, the self and society. Pt. i. gives a record of ingeniously devised experiments which, under a certain degree of mathematical torture, yield a quantitative formulation of normal suggestibility. A comparison of their results with hypnotic phenomena brings out the difference between normal and abnormal suggestibility: the former varies as indirect suggestion, and inversely as direct suggestion; the latter the reverse. Suggestibility at large varies as the amount of disaggregation, and inversely as the unification of consciousness.

Pt. ii. is the most elaborate and least satisfactory portion of the work. The datum of psychology, we are told, is the moment-contents forming the psychic concomitant of the activity of the single nerve cell. Functional association of cells is paralleled by synthesis of consciousness. We have, first, desultory consciousness, the "psychic life of the lowest invertebrates". Then comes reproductive or synthetic consciousness, which is instinctive, impersonal experience. Above this stands recognitive consciousness; memory still without personality, the mentality of the higher vertebrate animals. All these three stages are stages of the subconsciousness. Now we rise to desultory self-consciousness, serial or discontinuous personality. Next in order follows synthetic self-consciousness, "represented by man's mental activity"; and, finally, we reach, in the eternal moment of self-consciousness, the hypothetical perfect person. The subconscious self, looked at as a whole, is "essentially a brutal self". Indeed, it is not a 'self' at all, but merely a consciousness; it has no personality; it is stupid, uncritical, non-moral, associative,—everything that the true personality is not.

It is not probable that the 'normal' psychologist will accept this classification. We may grant all that can be granted to the current theory of mental "dissociation" or "sejunction" and its cellular parallel, but we may not ignore the established facts of mental mechanics. The author seems hardly to realise that normal psychology exists outside of Professor James' two volumes, which themselves interest him more in their discussion of the abnormal than of the normal mind. The consequence is an entire failure to see his problem in perspective. A knowledge of experimental literature would have helped him very considerably in pt. i.; similar knowledge would have shown him the difficulties and inadequacies of the view he has put forward in pt. ii. Normal psychology has a good deal to say about the mechanism of memory, of recognition, of impulse, of association, as genetic psychology has about the relation of 'lower' minds to 'higher'. You cannot get the adult human mind out of the cell mind by a merely additive aggregation, even if you declare your conviction (in a footnote) that "the very nature of mental activity is *synthesis*". Nor can you replace the work accomplished through analytic introspection by a list of subconscious states: "hypnotic, somnambulic, hypnonergic, hypnoid, hypnoidic, hypnoidal, hypnoleptic," however long you make it. It is the old mistake: the author takes his

pathological literature and pathological cases as things apart, and works them over with great pains and ingenuity, arriving at little or no result of general validity for the simple reason that normal psychology, the legislative science, is left out of account.

Pt. iii. is written in lighter vein. It gives a slight historical sketch of social epidemics, stampedes, social crazes, from the first Crusade to the present time, and draws certain conclusions as to social suggestibility, with especial reference to American society. "American society oscillates between acute financial mania and attacks of religious insanity. . . . [It] *seems to suffer from circular insanity.*"

Dr. Sidis writes vigorously, with the full courage of his convictions. He makes definite contributions to mental pathology, and sets many old facts in a new light. The book is concerned with matters that appeal both to the psychologist and to the neurologist, and would, undoubtedly, have got itself discussed though it had lacked Professor James' Introduction. But the presence of such an Introduction, even from so eminent a psychologist, cannot compensate for lack of psychological knowledge on the part of the author. And lack of comparative knowledge, want of perspective, is the weakness of the book.

E. B. TITCHENER.

The Study of Children and Their School Training. By FRANCIS WARNER. New York and London: The Macmillan Co., 1897. Pp. xix., 264.

"This book has been written," says Dr. Warner in his Preface, "in the hope of aiding an advance in the care of children, and in the practice of educational methods, by promoting a more exact study and classification of the children to be cared for and trained; while giving an account of some conditions of childhood in its many varieties, as seen from the standpoint of the observer who records what he sees as in other branches of physical science." The range of the author's experience is attested by the following statements: "I shall here use points for observations which I began to study twenty years ago. . . . In 1888 a committee was formed by the British Medical Association to study school children as to their mental and physical status, and . . . I was enabled to examine individually 100,000 children upon a fixed plan."

The volume is, then, not so much a contribution to child study, in the ordinary psychological sense of that phrase, as it is to child anthropometry and child hygiene. There can be no doubt of its utility to parents and teachers; not only are the things that Dr. Warner tells them intrinsically worth knowing, but they are things that must be known before the study of child psychology can begin. *Nemo psychologus nisi*—so far as these facts go—*physiologus.*

After a general introduction come two chapters on the growth and construction of the child's body, and the development of its brain. Four chapters deal with methods of observation, and the bodily and mental examinations to be made. Ch. viii. discusses such conditions as sleep, inattention, irritability, excitement. Ch. ix. distinguishes the types of childhood. Ch. x. treats of adolescence. Chs. xi. and xii. give instructions for care and training, hygiene and health management. A final chapter generalises the author's conclusions in the form of seven "propositions concerning childhood". The book contains reports of fifty cases, and has nineteen figures and eight tables inserted in the text.

Evolutional Ethics and Animal Psychology. By E. P. Evans. New York: D. Appleton & Co., 1898. Pp. vi., 386.

The central thesis of this book—for the most part a revised and enlarged reprint of previously published magazine articles—is that the animal mind differs from the human in degree only, and that the measure of man's moral duty to the animals is determined by the amount of this gradual difference. Pt. i., evolutional ethics, contains chapters on the ethics of tribal society, religious belief as a basis of moral obligation, the ethical relations of man to beast, and metempsychosis. Pt. ii., animal psychology, treats of mind in man and brute, progress and perfectibility in the lower animals, ideation in animals and men, speech as a barrier between man and beast, and the æsthetic sense and religious sentiment in animals.

The essays are written in a chatty, desultory fashion, without much regard to the logical difficulties and requirements of the theory of mental and moral evolution. Sir Henry Maine is followed in the first Part, Prof. Max Müller dissented from in the second. The author would probably deprecate technical criticism, and defend the anecdotal method as that best adapted to his purpose, the convincing of the average man. He has read widely, if not always discriminatingly (*cf.* the nine page bibliography that ends the volume). At times, however, he is not very scrupulous in the use of material and the weighing of evidence; there are quotations from early editions which have been expressly disowned by their authors, and stories are told as true which the expert has long since ceased to take seriously.

The Method of Darwin: A Study in Scientific Method. By Frank Cramer. Chicago: A. C. McClurg & Co., 1896. Pp. 232.

In a review of the *Origin of Species*, republished in *Lay Sermons*, the late Professor Huxley took occasion to show that Darwin's method satisfied the requirements of scientific logic, as laid down by Mill. Elsewhere in the same volume he emphasises the value of the natural history sciences for mental discipline, for training in logical method. In the book before us Mr. Cramer has chosen Darwin as an instance of the "best practice in matters of reasoning," and made his works the basis of an analysis of scientific method. In justifying his choice of a model the author rightly says that "Darwin's custom of presenting all sides of a case very frequently led him to expose the original course of his thought".

The book contains chapters on Darwin's own views of method; on classification, analogy, deduction, induction, etc., and on the logical history of the principle of natural selection. Each topic is discussed in the light of a number of examples, all of them labelled with page, volume and edition of the original works. The reader does not get, as indeed he should not expect, any very deep insight into the nature of logical processes; but he gets what is sound exposition, so far as it goes, presented in an extremely interesting way.

A Compendium of Insanity. By J. B. Chapin. Philadelphia: W. B Saunders, 1898. Pp. 234.

Dr. Chapin has compiled a useful practical manual of insanity for the assistance of physicians and medical students. He devotes most

of his space to a description of idiocy and imbecility, and of the four great types of positive insanity—mania, melancholia, dementia and general paresis; though epilepsy and the shock psychoses receive some notice. The difficulty and importance of a good working definition of insanity are well brought out, and plain directions given as regards the granting of certificates, the action of the expert in the courts and the detection of feigned insanity. An especially noteworthy feature of the book is the chapter on morbid anatomy, written in co-operation with Dr. M. Mosher.

Signs of haste are too evident in the work: witness the printing of identical paragraphs on pp. 193 and 209. The psychology of the Introduction is antiquated, but its statements are replaced by more modern conceptions in the body of the treatise. The book that shall base a discussion of insanity upon the established facts of the 'new' psychology is yet to be written.

Social and Ethical Interpretations in Mental Development. By JAMES MARK BALDWIN, Professor in Princeton University. New York: The Macmillan Co., 1897. Pp. xiv., 574.

This Study in Social Psychology is an important contribution to the literature which is accumulating concerning the relation of the individual to society. Prof. Baldwin follows the French school in laying much stress upon imitation as the fundamental process by which the social consciousness arises; and he describes at length the way in which the child enters into the social consciousness by what he also calls the process of social heredity. The part played by invention in social progress, the possibility of the genius in a world maintained by imitation, the sentiments and sanctions of the individual and the "matter of social organisation," are some of the subjects dealt with in this book. Full notice will follow.

L'Évolution des idées générales. Par TH. RIBOT, professeur au Collège de France, directeur de la *Revue Philosophique*. Paris: Félix Alcan. Pp. 260.

It is M. Ribot's intention to publish a series of treatises on various branches of psychology. This book, which is a summary of lectures given at the Collège de France in 1895, is the first of the proposed series, and deals with the processes of abstraction and generalisation. M. Ribot traces the development of general ideas in (1) animals, children and deaf mutes, (2) primitive races, (3) scientific theory and classification. This division corresponds to three stages: (1) the stage previous to language, (2) that at which ideas are accompanied by words and the importance of language gradually increases, (3) that at which the substitution of words for ideas is complete. There are, accordingly, two elements in conceptual thought: the conscious element, which is language with or without mental images, and the obscure, unconscious element, without which the process of symbolic thought would be entirely mechanical. A notice will follow.

E. F. STEVENSON.

Le Rationnel; Études complémentaires à L'Essai sur la Certitude Logique. Par GASTON MILHAUD, Agrégé de Mathématiques, Docteur ès lettres, Chargé de cours de philosophie à l'Université de Montpellier. Paris: Félix Alcan, 1898. Pp. 179.

The object of the group of studies collected in this small volume is to inquire into the nature of the logical or rational factor which (according to Dr. Milhaud) grows ever more and more prominent in human knowledge. "Can the distinction between this rational factor and that which is merely empirical, between clear and intelligible ideas on the one hand, and facts as presented to perception in all their complexity on the other hand,—can this be ultimately reduced to the simple distinction between abstract and concrete? Is there a difference of degree only, among the mental processes which extend from the point at which the mind first apprehends the impressions presented to it, from the simple reception of the given data, to the most lofty conceptions of speculative science? In this labour, which engrosses thought, has the mind an active and unique *rôle*—does it show some measure of spontaneity,—or does it simply disentangle, from a reality which dominates it, the intelligible notions by means of which that reality is known? Up to what point does the stamp of this rationality extend? What is the sense in which reasoned thought represents the things which reason helps to render comprehensible? and within what limits is it itself determined by those things? . . . These are the questions which, in one form or another, I ask on every page. It is unnecessary to say that I make no pretension to have solved them. My only demand is, that, in seeking a theory of rational knowledge, more account should be taken than usually is, of a spontaneous activity of mind, and that we should not be afraid of going so far as to recognise in this creative activity some degree of contingency and indeterminateness" (*Le Rationnel*, pp. 2, 3).

The six studies of which this book consists are described by Dr. Milhaud as complementary to his *Essai sur la Certitude Logique* published in 1894, and noticed in MIND for July of that year. The general position of the author, as indicated in the Essay, appears unmodified in *Le Rationnel*—he still draws the sharpest distinction between reason and reality, thought and thing, and explains the special character of mathematics—which from its logical certainty is the ideal of science—by the theory that mathematics is in a peculiar sense the construction of the mind, the work of pure thought, and that it transcends in a unique way the data given in experience.

The dualism of M. Milhaud's view seems to me to be somewhat crude and exaggerated, and the explanation of the peculiar character of mathematics to be essentially different from that here set forth. We may indeed acquire the power of carrying on mathematical thought without the presence of concrete objects, or visible symbols and figures; the lines or circles with which we work may be merely 'subjective,' 'rational,' merely *thought of* and not actually seen; and moreover thought of as absolutely straight, perfectly circular and so on; while the lines and circles which we draw and measure are found by the application of delicate instruments to be *not* perfectly straight or round. But what of that? The circles and triangles which ornament, *e.g.*, the pages of Euclid's *Elements*, are, *to our perception*, perfect examples of triangles and circles, and our thought-images are copies or counterparts of those seen, *as they appeared to us*. By reasoning similar to that which is used to show that none of the actual figures which we see and use in mathematical demonstrations are perfect, it might be shown that light and sound are only undulations, white light really parti-coloured, and so on.

But in spite of some general disagreement, a great deal of the detail of Dr. Milhaud's studies seems to me sound and excellent, and his views are set forth with all the attraction of lucid exposition and charming and polished style. His book is genuinely interesting—I found the sixth study (*Le raisonnement géométrique et le syllogisme*) particularly so—and his general view of the relation between geometrical reasoning and the syllogism seems to me admirable. He might however with advantage have taken a somewhat wider view of deductive mediate reasoning, and also have indicated the characteristic difference between equational (relative) and ordinary syllogisms. And as to the question whether knowledge can be added to by syllogistic reasoning, I think the difficulty is, to show that in passing *from the premisses taken together* to the conclusion there is *some* movement of thought, that there is *something* new in the conclusion. This it would have been easy for M. Milhaud to show in accordance with his general conception of the movement of thought in mediate reasoning. The point on which he insists—that new connexions are found and new knowledge gained *by bringing together two premisses not previously thought of in conjunction*—could hardly be matter of question. And I must remark that his conclusion (in accord with his general doctrine) that "we must do without a *logical* justification of syllogism," and that "the problem is essentially subjective," seems to me as mistaken as it is unsatisfying. This, and his discussion of the "Principle of Identity," might be amended by reference to the double force of terms as 'intensional' and 'extensional'.

The thesis of the first study (*Mathématique et philosophie*) is the part which scientific (mathematical) thought has played in the formation and evolution of philosophic doctrine; the second (*La science rationnelle*) examines in detail, and by the aid of numerous examples, the function of thought, theory, or hypothesis in science, beginning with laws commonly enounced as laws of the connexion of *facts*, and going on by degrees to what are recognised as scientific hypotheses. In the third study (*A propos de la géométrie grecque: une condition du progrès scientifique*) Dr. Milhaud insists that true science is speculative and disinterested, and that this disinterestedness, this detachment from practical utility, is the condition of scientific progress. The remaining studies are a note on Greek Geometry, and a dialogue on the notion of Limit in Mathematics.

E. E. C. J.

Dürfen wir den Ameisen und Bienen psychische Qualitäten zuschreiben? Von ALBRECHT BETHE. In *Pflüger's Archiv f. d. ges. Physiologie*, vol. lxx., Nos. 1 and 2. Bonn, 1898. Pp. 86, with 5 figures in the text and 2 plates.

This is a very important—negative—contribution to animal psychology. Briefly stated, the author's thesis is that psychical life takes its origin with the vertebrate series; that the invertebrates are endowed with no sensations, accumulate no experiences, and therefore show no modification of action; that they are, in a word, automata, reacting mechanically to stimuli which never pass the limen of sense perception. The essay contains an introduction and two chapters, dealing respectively with the behaviour of ants and bees.

The introduction falls into two parts. The first is critical and methodological. Wundt is taken to task for not following his own rule of simplest explanation; Haeckel and von Hartmann for neglecting facts

in their leap towards monism; Romanes for credulity; Weismann for his definition of instinct. The criterion of mentality is capacity to learn, to modify action—and, more than that, so to modify action that it becomes qualitatively distinct from the connate actions reflexly released by their adequate stimuli. The second part sketches the development of the reflex as determined by germinal variation (Weismann) and natural selection. These principles enable us to account for differences of internal organs and organic functions as well as of outward form; indeed, germinal variation allows of the "origination of the qualitatively new in the chemical economy of the individual". Passing then to a concrete illustration, the writer shows that there exist, within the cognisance of man's sense of smell, generic, specific, family and individual odours, so that one can identify a ruminant, a horse, the members of a given family, and even particular persons, by scent alone. This fact suggests that a volatile chemical substance may play a very considerable part, as the stimulus to reflex movements, in the life of the lower animals.

The chapter on ants opens with a careful experimental investigation of the question whether the members of a formicary recognise or 'know' one another. The conclusion is reached that each nest has its own 'nest-substance,' a volatile chemical substance (mixture of fatty acids?), alike or nearly alike for all members of the nest, and produced by the individual insect. The reaction to 'familiar' and 'unfamiliar' nest-substances is connate, not acquired. A similar inquiry into the mechanism of 'homing' shows that ants leave upon their path a volatile chemical slot, which is polarised, *i.e.*, differs according as the insect is travelling to or from the nest. The slot is 'received' through the antennæ, and releases the 'to' or 'from' movements reflexly. Finally, differential experiments prove that ants possess no means of 'communication'. Indeed, there is no evidence whatsoever of any modification of action that would justify our ascribing mentality to them.

Bees do not recognise the members of the hive, any more than ants the members of the ant-heap. Each hive has its 'nest-substance,' individually produced, and the treatment of 'friends' and 'foes' is a simple chemo-reflex, connate and founded on germinal variation. The homing of bees is a more difficult matter. It can be shown that neither the nest-substance of the hive, nor the slot of the insects in the air, is the chief determinant of the course of flight. Three hypotheses remain: that of memory, that of influence by terrestrial magnetism, and that of nervous registration of movement (orientation mechanism). None of these will 'work'; acoustic, magnetic, chemical and optical stimuli, as well as memory images, fail to yield an explanation of the facts. The author decides that 'a wholly unknown force' is operative in the case. It impels the bees to return to the point in space (not necessarily the hive) from which they set out, and is effective over a circular area of some 3 or 4 km. radius. But neither the fact of homing, nor any other fact in the bee life, constrains us to posit an insect mind.

Dr. Bethe's experiments, of which no description can be given in a brief notice of his essay, are admirably conducted; I know of none that surpass and of few that equal them in this field. His record of procedure is also full and careful, so that they can easily be repeated. His conclusion that none of the results furnish a proof of the presence of mental processes—a conclusion that is cautiously formulated, though the writer's personal belief plainly far outruns it—can hardly be denied. There are, however, several points in the two chapters that need further investigation; notably certain of the 'polarisation' phenomena of chap. i., and the nature of the *uns gänzlich unbekannte Kraft* of chap. ii.

In his reasoning on general principles the author is less happy. His criticism of Wundt, from the standpoint of a theory of the reflex which Wundt (and with him, probably, the majority of experimental psychologists) would not for a moment accept, may even be characterised as naïve. The strength of the paper lies partly in the rigour of its method, partly in the evidence which it brings of a reaction towards automatism as the explanation of a great body of animal actions. In the latter regard it may be compared with Prof. Loeb's article on instinct, published in the July, 1897, number of the *Monist*.

E. B. TITCHENER.

Die Ethik der alten Stoa, untersucht von Dr. ADOLF DYROFF. (Berliner Studien für Classische Philologie und Archaeologie.) Calvary & Co., Berlin, 1897. Pp. xvi., 410.

Dr. Dyroff deserves the thanks of every student of ancient philosophy for this admirable account of the ethical teaching of earlier Stoicism. It is by far the most complete and scholarly work upon the subject which we have seen, and forms a welcome supplement and companion to Pearson's well-known collection of *The Fragments of Zeno and Cleanthes* (1891). After a short introduction containing a general view of the leading characteristics of Stoic ethical theory, Dr. Dyroff proceeds to expound "die allgemeine Ethik" according to the sequence of topics observed by the founders of the Stoic school. Impulse (*ὁρμή*), the Ethical End (*τέλος*), Goods, Evils, and Intermediates (*ἀγαθά, κακά, οὐδέτερα*), Actions (*πράξεις*), and Emotions (*πάθη*) are exhaustively discussed. The third chapter (pp. 181-314) treats of various questions of practical or "hortatory" ethics, in particular of the Stoic attitude to politics, law, government and education. In a concluding chapter (pp. 315-337), the author determines the historical position of earlier Stoic teaching, laying stress, as has often been done before, on the influence of Oriental thought in determining the tone, though not the content, of the system. A few paragraphs are added on the relation of Stoicism to Christianity. Finally, we have a number of Appendices, in which many points of interest are further elucidated, such as the relation of the Porch to earlier schools, the position of Aristo of Chios, and Stoic views on Providence. The work ends with excellent indices, and a table of contents.

Dr. Dyroff's exposition is always clear, concise, and forcible. He has a thorough knowledge, not merely of the ancient sources, but of all the most important contributions to the history of Stoic ethics in recent times. His two outstanding merits are, as it appears to us, a remarkable faculty of seizing and describing the historical evolution of dogma, and a singularly acute and sound scholarship. The latter feature is all the more welcome, because it is by no means universal among German writers on ancient philosophy. As examples we may cite the author's attempt to prove that the pseudo-Plutarch's tractate on Education is based upon a work by Chrysippus (pp. 238 ff.). Here and there, we think, Dr. Dyroff presses his argument too far. Thus on p. 267 he finds an indication of Chrysippus in the following words of pseudo-Plutarch (3 *e*): *ὥσπερ γὰρ τὰ μέλη τοῦ σώματος εὐθὺς ἀπὸ γενέσεως πλάττειν τῶν τέκνων ἀναγκαῖόν ἐστιν, ἵνα ταῦτ' ὀρθὰ καὶ ἀστραβῆ φύηται, τὸν αὐτὸν τρόπον ἐξ ἀρχῆς τὰ τῶν τέκνων ἤθη ῥυθμίζειν προσήκει.* The same illustration was no doubt used by Chrysippus, but to us the passage forcibly recalls the words of Plato (*Rep.*, ii., 377 *c*): *πλάττειν τὰς ψυχὰς αὐτῶν τοῖς μύθοις πολὺ μᾶλλον ἢ τὰ σώματα ταῖς χερσίν.* The author of the treatise ascribed to Plutarch may well have derived the simile from Plato direct. But in general Dr.

Dyroff's reasoning is eminently sober and judicious. As examples of his excellent scholarship, we may refer to pp. 31 and 34, where *ὁμολογουμένως ζῆν* is shown to contain an etymological play (*ὁμο-λόγου-μένως*), by reference to the gloss in Stobæus: *τοῦτο δ' ἐστὶ καθ' ἕνα λόγον καὶ συμφώνως ζῆν ὡς τῶν μαχομένως ζώντων κακοδαιμονούντων.* The author's observations on Stoic etymologising (pp. 92 ff.) are remarkably acute and interesting. So likewise is his account of the process known as *μεταγράφειν* or *ἐπανορθοῦσθαι*—the emendation of well-known verses and quotations in order to communicate a moral lesson; as, for example, when the epitaph on Sardanapalus,

κεῖν' ἔχω ὅσσ' ἔφαγον καὶ ἐφύβρισα καὶ σὺν ἔρωτι
τέρπν' ἔπαθον· τὰ δὲ πολλὰ καὶ ὄλβια πάντα λέλυνται (? λέλειπται),

became in the hands of Crates,

ταῦτ' ἔχω ὅσσ' ἔμαθον καὶ ἐφρόντισα καὶ μετὰ Μουσῶν
σέμν' ἐδάην· τὰ δὲ πολλὰ καὶ ὄλβια τῦφος ἔμαρψε.

We have said enough to indicate the scope and character of Dr. Dyroff's work. The whole volume is full of interest to students of philology as well as of philosophy, and deserves the attention of all who are interested in the history of ethical theory and dogma.

J. Adam.

Received also:—

H. C. Bastian, *Aphasia and other Speech Defects*, London, H. K. Lewis, 1898, pp. viii., 366.

E. B. Titchener, *A Primer of Psychology*, New York, The Macmillan Company; London, Macmillan & Co., Ltd., 1898, pp. ix., 314.

M. E. Lowndes, *Michel de Montaigne*, Cambridge, The University Press, 1898, pp. xiv., 286.

W. N. Clarke, *An Outline of Christian Theology*, New York, Chas. Scribner's Sons, 1898, pp. ix., 488.

J. Royce, *Studies of Good and Evil*, New York, D. Appleton & Co., 1898, pp. xv., 384.

A. Fouillée, *Psychologie du Peuple Français*, Paris, Félix Alcan, 1898, pp. iv., 388.

E. S. Y. Escartin, *L'individu et la Réforme Sociale*, Paris, Félix Alcan, 1898, pp. viii., 390.

F. Paulsen, *Immanuel Kant*, sein Leben und seine Lehre, Stuttgart Fr. Frommanns Verlag (E. Hauff), 1898, pp. xii., 396.

R. Schellwien, *Philosophie und Leben*, London, Williams & Norgate, 1898, pp. 121.

C. E. Rasius, *Rechte und Pflichtender Kritik*, London, Williams & Norgate, 1898, pp. vi., 170.

S. Fragapane, *Obbietto e Limiti della Filosofia del Diritto* (*I.*), Roma, E. Loescher & Co., 1897; London, Williams & Norgate, pp. 156.

VIII.—PHILOSOPHICAL PERIODICALS.

PHILOSOPHICAL REVIEW. Vol. vii., No. 2. **J. Watson.** 'The Metaphysic of Aristotle.—II.' [For Aristotle, "the first principles of knowledge as well as those of reality belong to metaphysics, since apart from them there can be no knowledge of any reality". The process of knowledge is "a gradual ascent to first principles in which the ultimate presuppositions of all knowledge and reality at last come to direct consciousness". Metaphysics is thus a systematic exposition of the forms of intelligence itself. By the principle of contradiction Aristotle means to show that "the unity of intelligence with itself in conception, and the substantial reality and distinction of things, stand and fall together". (Criticism of Heraclitus and Protagoras.) By that of excluded middle he shows that "thought can grasp the real only if it comprehends the combination or separation of its elements, and that there cannot be anything real in which there is no such combination and separation". (Criticism of Anaxagoras.)] **J. G. Schurman.** 'The Genesis of the Critical Philosophy.—II. Psychological.' [Kant reached the standpoint of criticism by way of dogmatism (1746-60) and scepticism (1760-69). In the first period he still stands in the school of Leibniz and Wolff, and till 1755 is mainly occupied with physical science. The apostasy from Wolff results from the discoveries of 1761-66, that logical thought is analytic, that existence is no predicate of a thing, that causality is inexplicable to reason and (if not illusory) must be accepted from experience, that the methods of metaphysics and mathematics are wholly unlike, and that there is no knowledge of the suprasensible, metaphysics finding its chance of survival only as theory of the limits of human knowledge. Much as this table of conclusions simulates Hume's position in the Inquiry, Kant reached his standpoint independently, and is still not free from rationalism. The 'new method' (mentioned in 1765, dating really at least from 1762) is the obverse of the mediation of dogmatism: "there the procedure was to find identity amid differences, here it is to develop differences out of identity". The antinomy of pure reason forced Kant to a critique of reason itself: this is the *Umkippung* of 1769.] **H. Davies.** 'The Psychology of Temperament and its Epistemological Applications.' [The facts: temperaments in infancy and in mature life, special (artistic, critical, etc.), racial and class temperaments. The problem: physiological basis is to be sought in habit, psychological explanation runs in terms of attention, interest, individual variation. Temperaments, then, are referable, "on the one hand, to the ever-varying results wrought out in the body and due to some principle of heredity in transmission; and, on the other, to the functional activity of the individual, habitually reacting in attention and interest, on occasion of the stimulus of the environment, in harmony with a principle of unity". Epistemological bearings: Croom Robertson on heredity, influence of mood on activity of knowledge, the social factor in knowledge, feeling and theory of perception, knowledge as "the peculiar possession and development of the bents of feeling and temperament of the individual, subject to a teleological principle".—A thin paper.] Reviews of Books. Summaries of Articles. Notices of New Books. Notes.

PSYCHOLOGICAL REVIEW. Vol. v., No. 2. **J. Royce.** 'The Psychology of Invention.' [Subjects were required (1) to draw ten figures, not

imitative, as quickly as possible; (2) to draw ten original designs deliberately; (3) to draw quickly, at sight of ten figures, figures as unlike these as possible; and (4) to repeat this last series, but deliberately and comparatively. The effects of the 'social stimulation' in (3) led to the distinction of types, the self-preservative and the receptive. Upon the latter the intruding stimulus works in three ways: it produces a general tendency to vary; it induces a cautious and critical attitude, and so inhibits variation; it leads to a blending of the old style with the new, to true invention. The results show in miniature that "the social situation of the sort prevalent in an individualistic community involves altogether three kinds of motive," that to variation, that to selection, and that to the combination of the two.] **L. Farrand.** 'Proceedings of the Sixth Annual Meeting of the American Psychological Association, Ithaca, N.Y., December, 1897.' Discussion and reports. **J. Jastrow, J. M. Baldwin, J. M'K. Cattell.** 'Physical and Mental Tests.' [Aim is "to establish the normal capacity of simple and typical sensory, motor and intellectual endowments, as they occur in the average individual or in specially selected groups". Three problems: senses (sensitivity and sensible discrimination), motor capacities, complex processes (Jastrow). Investigation of memory is important (Baldwin). Report of committee and general criticism (Cattell).] **J. R. Angell.** 'Habit and Attention.' ["The reaction experiment represents an act in which are united two subordinate groups of habitual co-ordinations, one at the sensory end of the arc and one at the motor end." "The function of attention will be found at that point where the various co-ordinations comprised in the total sensation-and-movement act are least thoroughly habitual." The co-ordinations are thus susceptible to "the same kind of training which characterises all growth in habit".] **H. Davies.** 'The Trans-subjective as Psychological Fact.' [For Ladd against Miller.] **W. J.** 'Consciousness under Nitrous Oxide.' [Report of case by a philosophical student.] Psychological Literature. New Books. Notes.

Revue Philosophique. April, 1898. **Ch. Richet.** 'La forme et la durée de la vibration nerveuse et l'unité psychologique du temps.' [In the course of experimentation on dogs the writer was led to observe that each member of a uniform series of electric or other shocks is followed by uniform muscular response only if there is a certain minimum interval of time between each one. Response to a shock, that is to say, is followed by a 'période refractaire,' during which the nervous system is incapable of responding to stimuli. The facts observed can but be explained if we assume that in the response of the nervous system to stimulus we have a case of molecular vibration, and the author concludes that the time taken by the vibration and the return of the system to equilibrium is identical with the psychological unit of time.] **Winiarski.** 'Essai sur la mécanique sociale.' [I. Theory of economic and social equilibrium. II. Transformations of social energy. III. Social dynamics.] **G. Dumas.** 'L'état mentale d'Auguste Comte (conclusion).' Analyses et comptes rendus. Revue.

May, 1898. **Dunan.** 'La philosophie spiritualiste.' [Underlying all philosophies is one universal philosophy which is necessarily true and the content of which, though to a certain extent indeterminate, is expressed in the dogmas of the sanctity and inviolability of justice, the spirituality, liberty and immortality of the human soul and the existence of a powerful, good and wise God. Spiritualism (in this sense) is a form of instinct, which is defined as a natural disposition of all beings to exercise their functions and to accomplish those acts by means of which they can realise themselves. Since it is a function of the human mind

to understand as well as to believe efforts to systematise these ideas must always be made, and though every system must be imperfect, owing to the inherent limitations of human nature, yet philosophic speculation is not a game but an exercise of the highest and most human of our faculties, *i.e.*, reflexion.] **Martin (Abbé T.).** 'L'illusion des philosophes.' [Consisting in the fallacy of supposing that a philosopher's consciousness consists of two independent energies, one of which constructs or perceives the system he expounds, which the other can examine and judge disinterestedly.] **Calerion.** 'Sur la définition des grandeurs.' [Observations and documents.] **Dugas.** 'A case of depersonalisation.' Analyses et comptes rendus. Revue.

June, 1898. **Dr. E. Fardieu.** 'Psychologie du malade.' [Illness is a psychological leveller. It produces a distinct type of its own.] **R. de la Grassein.** 'La catégorie psychologique de la classification, révélée par le langage.' [Classification is concrete or abstract. The different abstract classifications are as follows: (1) *vitaliste* (animate or inanimate); (2) *rationaliste* (with or without reason); (3) *hoministe* (human or non-human); (4) *viriliste* (human male or other beings); (5) *intensiviste* (strong or weak); (6) *gradualiste* (diminutive or augmentative); (7) *masculiniste* (male or any other being); (8) *sexualiste* (male, female or asexual).] **Sikorsby.** 'Quelques traits de la psychologie des slaves.' Observations and documents. 'Memory in dreams.' Analyses et comptes rendus. Revue.

Revue de Métaphysique et de Morale. 6e Année, No. 2. March, 1898. **J. Lagneau.** 'Fragments.' [These fragments are prefaced with a letter which clearly sets forth what Spinoza means in the proposition that '*the body is the soul viewed on the side of extension*, and the soul on its part *is the idea of the body*'; a proposition which of course makes short work of the reality of purposiveness, as well as of personality; reducing them, even in ourselves, to little better than an illusion, persistent for all except Spinozistic philosophers. Spinoza's meaning is well expressed: and there is, besides, much independent thinking on Spinoza's lines, conducted in the light of subsequent philosophy. The writer (E. Chartier) promises to publish hereafter a commentary for the guidance of students: meanwhile he presents the 'Fragments' in a disjointed and unsystematised form. We must observe that in his prefatory letter Lagneau declares that Spinozism is true only in a limited sense, to be afterwards explained.] **Jacob.** 'La philosophie d'hier et celle d'aujourd'hui.' [The writer argues that the older philosophy, whose tendency to disappearance he regrets, promised better than its successor for the cultivation of the human mind and the attainment of truth. The older philosophy was, to some extent, still under the influence of the schools: it still employed the venerable conceptions of substance, of genera and species, and of 'final causes'. The newer is that of Bain, Mill, Spencer—the English thinkers and their French disciples. An interesting discussion follows, in which the weaknesses of the 'newer philosophy' are examined and exposed. This newer philosophy represents no real progress, but only a retrograde movement. It destroys, and does not create.] **G. Tarde.** 'Les lois sociales' (*suite*). [This follows up the article of January last, already noticed. Progress in the sciences generally has largely consisted in displacing a few empty and superficial, but strongly marked, conceptions of oppositions in nature, such as those of day-night, heaven-earth (in astronomy); fire-water, earth-air (in physics); health-sickness, and so on; and substituting for them a countless number of subtle and profound oppositions, discovered after painful research: replacing external and

apparent by internal and real distinctions. In Sociology the case is analogous. National oppositions (especially since the recent 'Europeanisation' of Japan) are no longer made so much of as they used to be by (*e.g.*) Plato, Hegel, Cousin, etc. Economists no longer treat wars as the key to history: the true key is to be found in the competition of desires and beliefs—the true forms or elements of rivalry and opposition to be sought for in the bosom of the social individual. But the 'opposition-struggle,' in its various phases, plays only the part of a 'middle term' in the development of humanity, and is destined to disappear finally, after founding a better state of things. It is, after all, only a means towards the 'adaptation of man to his environment']. Etudes Critiques, etc.

ZEITSCHRIFT FÜR PSYCHOLOGIE UND PHYSIOLOGIE DER SINNESORGANE. Bd. xvi., Heft 4. **R. Wahle.** 'Ueber den gegenwärtigen Zustand der Psychologie.' [Critique of three typical books (Jodl, Wundt, Ebbinghaus) on the ground of their underlying ontology. Jodl misuses the concept of consciousness, in making it a 'unity' within which 'elements' or 'moments' are distinguishable; he goes astray also in his doctrine of the antithesis of subject and object as characteristic of consciousness, in his notion of the elementary ego-form, in his identification of act, excitation, function, phenomenon, activity. There is no 'relating' of sensation to brain disturbance; the two occurrences are of precisely the same order. Wundt is not clear about the primary will process, and self-contradictory as regards the feelings. The concept of fusion is unscientific. Ebbinghaus has begun well; but may go wrong upon feeling and 'combination' later.] **Guillery.** 'Bemerkungen über Raum und Lichtsinn.' [Reply to Asher. Discusses the like and unlike conditions of the 'light sense' and the 'space sense' of the eye.] **R. Sommer.** 'Dreidimensionale Analyse von Ausdrucksbewegungen.' [Description of apparatus for recording hand-movements in the three planes of space. Useful for normal work (fatigue, etc.), for differentiating tremors (alcoholic, hysteric, paralytic), for the psychophysiology of 'thought reading,' etc. Record of experiments.] **J. Loeb.** 'Ueber Kontrasterscheinungen in Gebiete der Raumempfindungen.' [Defence of concept of space contrast, against Lipps.] Litteraturbericht.

Bd. xvi., Heft 5 und 6. **J. Hirschberg.** 'Die Optik der alten Griechen.' [The rectilineal propagation of light was understood, and its linear perspective given a correct geometrical exposition. The law of reflexion was known; the images of spherical mirrors presented difficulties. Good experiments on refraction were made, but the law did not attain mathematical formulation. As for physiological optics: fixation point and extension of the field of vision were familiar concepts. The law of binocular (single and double) vision was approximately known. Observations were made, and explanations attempted, of optical illusions.] **M. Meyer.** 'Ueber die Unterschiedsempfindlichkeit für Tonhöhen, nebst einigen Bemerkungen über die Methode der Minimaländerungen.' [Method of r. and w. cases: collective experiments with the tone 600, experiments on Stumpf with the tones 100, 200, 400, 600, 1200. Luft's results are approximately confirmed. (The method of work is excellent.) The judgment of 'difference,' without statement of direction, depends on secondary, *i.e.*, non-qualitative criteria. The method of minimal changes is unreliable: it is full of suggestion to the subject, and the course of the expectation-error cannot be predicted. (The criticism is written in an arrogant and polemical tone; and, moreover, does not take account of some of Wundt's rules for the use of the method.)] **W. A. Nagel.** 'Ueber das Aubertsche Phänomen und verwandte Täuschungen über die vertikale Richtung.' [If a vertical line of light be observed in the dark

room with the head inclined to the shoulder, it takes on a distinctly oblique position. Repetition and variation of this experiment: illusions of movement: experiments with passage of a constant current through the occiput. Theory: the compensatory torsions, useful in animals, are in man a useless and more or less vestigial reflex. They are probably concerned in the phenomenon under discussion; *cf.* Exner's autokinetic sensations. The *rôle* of the static sense is uncertain.] Besprechungen. [Lipps on Stout's *Analytic Psychology*, Lange on Helwig's *Theorie des Schönen*, Hoefler on von Stein's *Aesthetik*.] Litteraturbericht.

Bd. xvii., Heft 1 und 2. **M. Meyer.** 'Ueber die Intensitaet der Einzeltoene zusammengesetzter Klaenge.' [Mathematical formulation, with empirical verification, on the lines of the author's theory of hearing sketched in the preceding volume. An appendix figures a working model of wave-analysis by the cochlea according to the same theory.] **W. Filehne.** 'Die geometrisch-optischen Taeuschungen als Nachwirkungen der im koerperlichen Sehen erwerbenen Erfahrung.' [Normal vision is spatial vision. Illusions are due to (often subliminal) spatial associations; the figures contain 'perspective motives'. Analysis of Zoellner's figure, angle illusions, the Milton-Bradley (shifted chessboard) illusion, Loeb's illusion. Motives of perspective and of movement. An interesting paper, whose figures are especially valuable. The question of optical illusions is now on the way towards settlement. It is noteworthy that Filehne has worked independently of Einthoven and Wundt; Einthoven independently of Wundt; Wundt independently of Lipps.] **G. Heymans.** 'Zur Parallelismusfrage.' [Most opponents of the monistic 'two-side' theory have interpreted it as a form of the Spinozistic doctrine of substance and attribute. Over against this the author sets a parallelistic monism, which may be briefly formulated as follows: All reals (primary series) are psychical in nature. The only given reals are our conscious processes: the rest are gained by interpolation and extrapolation in kind. The indirect effects of reals on consciousness, effects produced under the conditions of perception, are the phenomena dealt with by natural science (secondary series). Both series are necessarily parallel, and form closed causal chains. Both are, evidently, psychical in nature, differing not in contents but in laws of succession. Critique of materialism and dualism, and comparison of them with this monism. Review of eight objections to the theory (Wentscher, Kroman, Erhardt, Hoefler). A clear paper. It should be remembered that a parallelistic monism need not be a psychical monism (this the writer points out); and that the interpretation of parallelism is but one out of many possible.] **F. Schumann.** 'Zur Psychologie der Zeitanschauung.' [Introduction to a promised series of experimental articles. Mueller's general theory of time perception. The verdict of introspection: form-qualities, comparing activity, inclusiveness of judgment, simultaneity of elements in consciousness, are needless. Critique of Stern (psychical present), Meinong and Witasek (consolidated contents), Wundt and Meumann. Result: direct temporal perception must be analysed as contents followed by judgment-formula—the contents, given in time relations, form a unitary whole, in the sense that they influence as a whole the course of association, feeling and judgment.] Litteraturbericht.

PHILOSOPHISCHE STUDIEN. Bd. xiii., Heft 4. **P. Mentz.** 'Untersuchungen zur Psychophysik der Farbenempfindungen am Spectrum, i.' [First part of what promises to be a very thorough and valuable investigation. Discusses the three variables, colour-tone, colour-degree, brightness-degree, in their connexion with language, and shows the

need and manner of their independent variation; describes the spectrometric apparatus employed; and gives eighty-eight values of the difference limen between the limits 773,994 and 396,862 $\mu\mu$ of an objective spectrum.] **G. F. Lipps.** 'Ueber Fechner's Collectivmaasslehre und die Vertheilungsgesetze der Collectivgegenstaende.' [Brief analysis, by the editor, of Fechner's posthumous work. The scientific advance in the book is the recognition of the interrelations of the theory of error and the theory of collective measurement. The fundamental problem of both theories is the development of a law of distribution that is approximately valid for a given collective object.] **G. Heymans.** 'Berichtigung.' [Correction of Wundt's account of his 'contrast-theory' of certain optical illusions. If we set out along a horizontal line from the acute angle formed by an inturned oblique piece, our attention is diverted by the latter: the length of the line is therefore shortened (Müller-Lyer), and the line itself appears to fall slightly from beginning to end (Zöllner).] **W. Wundt.** 'Bemerkungen zu vorstehender Berichtigung.' [Heymans is wrong in applying the illusions of movement (after-image, positive and negative) to a resting line, and in his idea of contrast, which is always a phenomenon of relation.]

Bd. xiv., Heft 1. **W. Wundt.** 'Zur Theorie der räumlichen Gesichtswahrnehmungen.' [A very important paper, whose object is the revision of current theories of visual space perception and of the facts upon which they rest. Acquaintance with the author's recent work on the geometrical optical illusions (*Abh. d. kgl. sächs. Ges. d. Wiss., math.-phys. Cl.*, xxiv., 2) is assumed throughout. (1) Wundt's ten years' experience of metamorphopsia points to the correctness of the genetic theory of space perception. The retina has the power of gradual adaptation to constant distortions of its image. (2) The work of Hillebrand and Arrer. The sensations accompanying convergence are not merely muscular sensations, in the strict sense of the word; they are muscular and tendinous sensations, plus the pseudo-articular sensations set up by rotation of the eye-ball in the orbit. They are not perceived as such, with small excursion, simply because they fuse to a whole, with other sense-elements, in perception: *cf.* Goldscheider's work on minimal, active and passive, arm-movements. (3) Geometrical optical illusions fall into four classes. The first is that of the reversible illusions of perspective,—illusions of great value, since in them the objective conditions of spatial form remain constant, and the perceptual variation must be due to the perceiving subject. An elaborate analysis (which cannot here be summarised) reduces the conditions of variation to position and movement of the eyes. The illusions serve well to illustrate the mechanics of the assimilation. Next in order come the variable illusions of distance and direction, with unequivocal secondary ideas of perspective. These are shown to depend upon the uniform interaction of retinal image and 'movement image'; the three laws of illusion can be directly deduced from three laws of eye-movement. In the third place stand the constant illusions of distance and direction, illusions that do not vary with variation of conditions. They are due to motor defects (*cf.* the dioptrical) of the visual apparatus. Lastly we have the associative illusions of approximation and contrast. They result from the mutual relations of perceptual contents, thus differing from the other three classes, whose illusions appear in the single perception. (4) The antithesis of nativism and empirism in its historical significance. Hering's nativistic theory posits three factors in space perception: a nativistic anatomical correlation of the two retinæ, a nativistic function of will or attention, and an empiristic 'experience'. The two first assumptions are evidently open to criticism,

and the second leads further to a onesidedly sensory view of the process of space perception in general. (5) Helmholtz' empiristic theory uses the three motives of touch, innervation-feelings round the eye, and retinal local signs. It fails, as every theory grounded on Lockian principles must fail, to get its space out of non-spatial contents. (6) There remains the genetic theory, or theory of complex local signs. This has, as its tools, a two-dimensional system of retinal local signs, and the one-dimensional of 'muscle' sensations. "The strain sensations of the eye, forming a continuum of one dimension, fuse associatively with the system of retinal local signs, a heterogeneous continuum of two dimensions, to produce a homogeneous continuum of two dimensions, *i.e.*, a space-superficies." The third dimension is given by the fact of binocular function; absolute orientation in space by reference to the body of the perceiving subject.—The net result of the paper is a very considerable strengthening of the genetic position. The observations on metamorphopsia are extremely noteworthy, in view of the part played by pathology in recent modifications of the nativistic theory. The new experiments on convergence have elicited a clear and convincing statement on the question of eye-movement sensations from the author of the 'eye-movement theory'. The mechanism of simultaneous association is laid bare, with the result of a solid gain to analytic psychology. Optical illusions are reduced to order. And the theory of complex local signs has received its definitive formulation.] **R. Richter.** 'Der Willensbegriff in der Lehre Spinoza's.' [I. The place of will in nature. (*a*) Nature as *naturans*.—The will of pt. i. of the Ethics covers both affirmation and desire. Metaphysical and logical proofs from the side of the idea of God, and systematic proofs from that of the idea of will, lead to the result that God is will-less. The test lies in the incorrectness of all ideas of end as referred to God. To maintain his position, Spinoza has to change the meaning of the attributes of omnipotence, creation and freedom; his idea of God is neither voluntaristic nor intellectual, but takes on a logical and causal colouring. (*b*) Nature as *naturata*.—It follows that will finds its place in the realm of modes, absolutely determined. It does not follow, though this is Spinoza's doctrine, that will is a finite (not, like understanding, both a finite and an infinite) mode. In the world of experience, idea has priority over will: the will in this case is will as desire. This principle, again, is not grounded on demonstration; set forth as axiom, it has led to the unproven substitution of part for whole, of idea for state of consciousness. Epistemology thus comes to stand for metaphysics.]

Vierteljahrsschrift für Wissenschaftliche Philosophie. Bd. xxii., Heft 2. **P. Barth.** 'Zum 100 Geburtstage Auguste Comte's.' [An interesting sketch of Comte's aims and achievements.] **Fr. Carstanjen.** 'Der Empiriokritizismus.' [Exposition of the psychology of Avenarius, with corrections of Wundt's statement.] Besprechungen, etc.

Archiv für Systematische Philosophie. Bd. iv., Heft 2. **E. Koch.** 'R. Avenarius' Kritik der reinen Erfahrung.' [Expository.] **J. Zahlfleisch.** 'Ueber Analogie und Phantasie.' [Importance of imaginative construction guided by analogy in the development of science, art and religion.] **S. Stein.** 'Wesen und Aufgabe der Sociologie.' [Sociology in its present state must be content to discover empirical rules, not apodeictic laws. Protest against Spencerian deduction from supposed biological analogies.] **F. Tönnies.** 'Jahresbericht ueber Erscheinungen der Sociologie aus den Jahren 1895 und 1896.'

RIVISTA ITALIANA DI FILOSOFIA. November-December. **F. de Sarlo.** 'Il Socialismo come Concezione Filosofica.' [It has been said that every one goes to the Bible to find his own opinions there, and so it seems to be to a large extent with Hegelianism. Certainly the Hegelian dialectic is here applied to the "conflict" between capital and labour, the higher unity being "Socialism, illumined or enlightened by the Hegelian Ethic". But if Socialism needs to be "illumined" obviously the dialectic moment is not necessary and internal, but external and artificial—apparently the same conflict might also find a higher unity, if not a remedy, in the wider distribution of profits and capital by further extensions of the system of Joint Stock Companies coupled with profit-sharing and bonuses. Besides if the "moment" is to be founded upon the thesis that capital is gradually working out its own extinction, through the profits on its use approaching the vanishing point, and if further this is exemplified by the fact that the *minimum* bank rate of discount in 1895 was only 2 per cent., one finds that, after all, the root of the Hegelian Dialectic was a temporary "glut of gold" in the city.] **B. Labanca.** 'La "Scienza Nuova" di Vico al lume della Bibbia in un raro libro del Seculo xviii.' **L. Ambrosi.** 'I Principi della bonoscenza e la loro prima Radice.' [A sequel to a former article entitled, "Le Creazioni dello Spirito Conoscenza Intellettiva". Mill's empirical account of Causality and the Uniformity of Nature is criticised as involving a vicious circle. Causality is necessary and involves the principles of Identity and Contradiction. Cognition ultimately results from intellectual activity, and the *momenta* may be summarised as follows: (*a*) We exist as cognitive; (*b*) something exists as cognised; (*c*) all things are intelligible, *i.e.*, there is objectivity, which is known. From these three principles result the three logical Laws—Space and Time (the first grade of the mental "hierarchy"); Substance and Cause (the second); and finally the principle of the Absolute (the third), *i.e.*, that the part presupposes the whole; the contingent the necessary, the imperfect the perfect, the finite the infinite, and the conditioned the unconditioned.] 'Seneca Filosofo e San Paolo.' [An elaborate account of Seneca's teachers and comparison of his opinions with those of St. Paul. (To be continued.)] **D. Grasso.** 'Studio sull' Attenzione.' [The opinions of Greek thinkers are omitted on account of their obscurity. The Cartesian and Scottish Schools are important as giving analyses of the *effects* of Attention (in treating of the latter it may be noted that the term "Scottish School" is confined to Reid and Stewart; if it were extended to include their followers, it would not be true that the "Scottish School" made attention a separate faculty). Wolff recorded the Phenomena, Condillac the Mechanism, Bonnet the Physiology, and Laromiguière the Genesis of Attention, and a detailed specification of these and other contributions to the subject shows how much more modern psychologists are indebted to earlier investigators.] Bollettino, etc.

IX.—NOTE.

ARISTOTELIAN SOCIETY.

The Nineteenth Session closed on 6th June. Prof. D. G. Ritchie was elected President in succession to Mr. Bernard Bosanquet, who retired. The new Session will open with the Presidential address on 4th November.

THE ARISTOTELIAN SOCIETY,

22 ALBEMARLE STREET, LONDON.

REPORT OF THE EXECUTIVE COMMITTEE FOR THE NINETEENTH SESSION.

Presented at the Last Meeting of the Session on 6th June, 1898.

THE work of the Society during the past Session has been fully maintained at a high level of philosophical interest. Nine of the papers read before the Society, and three critical notices of new Philosophical Books, have been accepted by *Mind*, and one paper, which exceeded the limits of an article, Mr. E. T. Dixon's "The Foundations of Projective Geometry," has been published by the author in book form. The number of the Society's papers accepted by *Mind* necessitated the printing of an extra sheet in the July number last year, and will necessitate a similar enlargement this year, the expense in each case is being borne by the Society. The Committee propose to arrange for the printing and circulating in proof of all papers next Session, the Society paying the expense of those which are not accepted for publication in *Mind*. An important consideration in connection with this proposal is the question of the length of papers, many of those read this Session have been too long for publication in one article. The Committee recognise that it is neither possible nor desirable to restrict the writer of a paper within a hard and fast limit, but suggest to members that a paper exceeding twenty pages of *Mind* in length is very difficult to read and adequately discuss at one meeting. A list of suggested subjects for papers is not added to this report, as it is thought that members prefer to select subjects from their own special studies.

FINANCIAL STATEMENT.

NINETEENTH SESSION, 1897-98.

RECEIPTS.				EXPENDITURE.			
Members' Subscriptions	£73 10 0			General Expenses—			
Less unpaid	7 7 0			Rent of Rooms	£14 14 0		
		£66 3 0		Printing Society Notices	8 16 6		
Arrears received		4 4 0		Advertising Meetings	0 6 0		
Sales of *Proceedings* in 1897		4 14 6		Gratuities	1 2 6		
Publication Fund—				Postage	5 12 11		
Brought forward	80 17 7					£30 11 11	
Interest on Deposit Account for 1897	2 0 10			Copies of *Mind* for July and Oct., 1897		17 16 6	
		82 18 5		Subscription to *Mind*, 1898		37 0 0	
Balance brought forward from last Session		1 14 10		Printing extra sheet of *Mind*, July, 1897		4 6 9	
Balance carried forward to next Session		12 18 10		Post Office Savings Bank—			
				Publication Fund		82 18 5	
		£172 13 7				£172 13 7	

Examined and found correct.

E. C. BENECKE.
F. KAIBEL.

H. WILDON CARR.
Hon. Treasurer.

Proceedings of the Aristotelian Society.
NINETEENTH SESSION.

Meetings at 22 Albemarle Street, at 8 p.m.

1897.

Nov. 1.—Mr. Bernard Bosanquet, President, in the Chair. Mr. E. Mooney was elected a member. The President delivered the inaugural address on "Hegel's Theory of the Political Organism".

Nov. 15.—Mr. A. Boutwood, V.-P., in the Chair. Mr. W. MacDougall and Mr. W. R. Boyce Gibson were elected members. Mr. G. E. Moore read a paper on "Freedom".

Nov. 29.—Mr. A. F. Shand, V.-P., in the Chair. Dr. Edward Westermarck was elected a member. Mr. W. MacDougall read a paper on "The Physiological Conditions of Consciousness".

Dec. 13.—Mr. Shadworth H. Hodgson, V.-P., in the Chair. Mr. E. T. Dixon read a paper on "The Foundations of Projective Geometry".

1898.

Jan. 17.—Mr. Shadworth H. Hodgson, V.-P., in the Chair. Miss Constance Jones read a paper on "The Paradox of Inference".

Jan. 31.—Mr. A. Boutwood, V.-P., in the Chair. Miss L. B. Bradby was elected a member. Mr. C. Llewelyn Davies, Mr. G. E. Moore and Dr. Stanton Coit took part in a symposium on "In what Sense, if any, must the Universe Satisfy the Demands of our Practical Reason?"

Feb. 14.—Mr. Shadworth H. Hodgson, V.-P., in the Chair. Mr. W. R. Boyce Gibson read a paper on "The Regulae of Descartes".

Feb. 28.—Mr. A. F. Shand, V.-P., in the Chair. Dr. Edward Westermarck read a paper on "The Essence of Revenge".

March 14.—Mr. Shadworth H. Hodgson, V.-P., in the Chair. A critical notice of *The Philosophical Lectures and Remains of Richard Lewis Nettleship*, edited by A. C. Bradley," by Mr. Bernard Bosanquet, was read by the Honorary Secretary. Mr. Bertrand Russell read a critical notice of "Love's *Theoretical Mechanics*".

March 28.—Mr. A. Boutwood, V.-P., in the Chair. Mr. A. F. Shand read a paper on "The Universal Constituents of Mind—Feeling, Thought and Conation".

April 25.—Mr. Shadworth H. Hodgson, V.-P., in the Chair. Prof. D. G. Ritchie read a paper on "The One and the Many".

May 9.—Mr. A. F. Shand, V.-P., in the Chair. Dr. G. Dawes Hicks read a paper on "Growth in Consciousness".

May 23.—Mr. A. Boutwood, V.-P., in the Chair. Mr. E. C. Benecke and Mr. F. Kaibel were appointed auditors. Mr. J. E. M'Taggart read a paper on "Hegel's Treatment of the Categories of the Objective Notion".

June 6.—Mr. Shadworth H. Hodgson, V.-P., in the Chair. The Honorary Secretary read the Report of the Executive Committee for the Session and the Financial Statement audited by Mr. E. C. Benecke and Mr. F. Kaibel. The officers for the ensuing Session were elected by ballot as follows: President, Prof. D. G. Ritchie. Vice-Presidents, Mr. A. Boutwood, Prof. J. H. Muirhead and Mr. A. F. Shand. Editor, Mr. G. F. Stout. Honorary Secretary and Treasurer, Mr. H. Wildon Carr. A resolution was proposed by Miss Mason, seconded by Miss Murray, and carried, "That the Society order, under Rule VIII., that every third meeting during the next Session he held from 5 to 7 P.M. instead of later in the evening, unless otherwise ordered by the Committee". A critical notice by Prof. J. Lindsay of M. H. Dziewicki's edition of *Johannis Wyclif Tractatus de Logica*, was read.

List of Officers and Members for the Twentieth Session, 1898-99.

President.

Prof. D. G. RITCHIE, M.A.

Vice-Presidents.

SHADWORTH H. HODGSON, M.A., LL.D.
(President, 1880 to 1894).

BERNARD BOSANQUET, M.A., LL.D.
(President, 1894 to 1898).

A. BOUTWOOD.

Prof. J. H. MUIRHEAD, M.A.

A. F. SHAND, M.A.

Editor.

G. F. STOUT, M.A.

Honorary Secretary.

H. WILDON CARR, 22 Albemarle Street, W.

Honorary and Corresponding Members.

	Date of Election.
Bain, Alexander, LL.D., Aberdeen	7*th Jan.*, 1884.
Elected Hon. Member	18*th Dec.*, 1893.
Cattell, J. M., M.A., Ph.D., University of Pennsylvania, United States	17*th June*, 1889.
Davidson, Thomas, Orange, New Jersey, United States	12*th Nov.*, 1883.
Dziewicki, M. H., 21 Szpitalna, Cracow, Austria	6*th June*, 1891.
Harris, William T., LL.D., Washington, United States	19*th Dec.*, 1881.
James, Prof. William, M.D., Cambridge, Mass., United States	5*th Feb.*, 1883.

Members.

	Date of Election.
ALEXANDER, Prof. SAMUEL, M.A., 13 Clifton Avenue, Fallowfield, Manchester	13*th April*, 1885.
ASKEW, J. B., 16 Cheyne Row, Chelsea, S.W.	22*nd Feb.*, 1897.
BANCKS, Rev. GERARD W., M.A., Durham House, Green Street Green, Dartford	17*th Feb.*, 1896.
BARNES, SHEPPERSON, Hospital Ship, Dartford, Kent	16*th March*, 1891.
BENECKE, E. C., 174 Denmark Hill, S.E.	18*th Dec.*, 1893.
BLUNT, H. W., M.A., Christ Church, Oxford	3*rd Dec.*, 1888.
BOSANQUET, BERNARD, M.A., LL.D., *Vice-Pres.*, The Birches, Caterham-on-the-Hill	22*nd Nov.*, 1886.
BOULTING, W., Guyon House, Heath Street, Hampstead	2*nd Dec.*, 1889.
BOUTWOOD, A., *Vice-Pres.*, Charity Commission, Whitehall, S.W.	12*th May*, 1890.
BRADBY, Miss L. BARBARA, 19 Linden Gardens, W.	31*st Jan.*, 1898.
BROUGH, Prof. J., LL.M., University College, Aberystwyth	29*th April*, 1889.
BRYANT, Mrs. SOPHIE, D.Sc., 12 Gayton Crescent, N.W.	7*th Jan.*, 1895.
BUTCHER, Prof. S. H., M.A., 27 Palmerston Pl., Edinburgh	10*th Dec.*, 1883.
CARR, H. W., *Hon. Sec.*, 25 Cumberland Terrace, Regent's Park	19*th Dec.*, 1881.
CHAMBERS, W. F. D., B.A., 9 Quentin Road, Blackheath	20*th May*, 1895.
COIT, STANTON, Ph.D., 9 Leighton Crescent, N.W.	4*th Nov.*, 1895.
CRAWLEY, A. E., M.A., St. John's School, Leatherhead	16*th Nov.*, 1896.
DANIELL, A. M., B.A., Saxifield, Filey Road, Scarborough	30*th Nov.*, 1891.
DAPHNE, P., LL.B., 11 Willow Bridge Road, Canonbury, N.	7*th Jan.*, 1884.
DAVIES, CROMPTON LLEWELYN, M.A., 14 Barton Street, Westminster	3*rd Dec.*, 1894.
DAVIS, C. T., 75 Cambridge Street, Eccleston Square	25*th Jan.*, 1897.
DAWSON, Miss M. E., 14 Nottingham Place, W.	14*th Dec.*, 1896.
DIXON, E. T., M.A., 9 Cranmer Road, Cambridge	17*th Feb.*, 1896.
DOWSON, Mrs., 91 Cheyne Walk, Chelsea	7*th Nov.*, 1890.
DUNSTAN, Prof. W. R., M.A., F.R.S., Queen Anne's Mansions, S.W.	19*th April*, 1880.
EVANS, Lady, Nash Mills, Hemel Hempstead	14*th Dec.*, 1891.
FAIRBROTHER, W. H., M.A., Lincoln College, Oxford	20*th Nov.*, 1893.
FLETCHER, Miss MARY, 9 Stanhope Street, Hyde Park Gardens	16*th Nov.*, 1896.
GIBSON, W. R. BOYCE, M.A., 1 Oak Terrace, Fairfield, Liverpool	15*th Nov.*, 1897.
GREECE, C. J., LL.D., Redhill, Surrey	9*th Oct.*, 1882.
HICKS, G. DAWES, M.A., Ph.D., 16 Aberdeen Road, Highbury, N.	17*th Nov.*, 1890.

	Date of Election.
HOBHOUSE, L. T., M.A., 1 Wynnstay Grove, Fallowfield, Manchester	11*th Jan.*, 1892.
HODGSON, SHADWORTH H., M.A., LL.D., *Vice-Pres.*, 45 Conduit Street, W.	18*th May*, 1880.
HOOPER, C. E., 34 Clifton Gardens, Maida Vale, W.	5*th April*, 1897.
HUSBAND, Mrs. GILLILAND, 8 Marlborough Road, N.W.	16*th Nov.*, 1896.
JACKSON, Miss L. M., 29 Manchester Street, W.	2*nd Nov.*, 1896.
JACOBS, JOSEPH, Merodelia, Grafton Road, Acton	16*th Nov.*, 1896.
JONES, Miss E. E. CONSTANCE, Girton College, Cambridge	19*th Dec.*, 1892.
KAIBEL, FREDERICK, Monument Buildings, E.C.	2*nd March*, 1896.
LAKE, A. F., 12 Park Hill, Clapham Park, S.W.	19*th Dec.*, 1881.
LINDSAY, Rev. JAMES, M.A., Springhill Terrace, Kilmarnock, N.B.	22*nd March*, 1897.
MACDOUGALL, W., M.A., St. Thomas's Hospital, S.E.	15*th Nov.*, 1897.
MACKENZIE, Prof. J. S., M.A., University College, Cardiff	20*th Nov.*, 1893.
MACTAGGART, J. ELLIS, M.A., Trinity College, Cambridge	16*th March*, 1896.
MANNING, Miss E. A., 35 Blomfield Road, Maida Hill	16*th Nov.*, 1896.
MASON, Miss F. Agnes, 79 West Cromwell Road, S.W.	11*th June*, 1888.
MASSEY, C. C., 124 Victoria Street, S.W.	10*th Dec.*, 1883.
MEYER, Miss B. E., 69 Longridge Road, Earl's Court, W.	8*th Feb.*, 1897.
MITCHESON, R. E., M.A., 4 Prince Arthur Road, Hampstead, N.W.	11*th March*, 1889.
MOONEY, E., 20 Applegarth Road, Brook Green, W.	1*st Nov.*, 1897.
MOORE, G. E., B.A., Trinity College, Cambridge	16*th Nov.*, 1896.
MUIRHEAD, Prof. J. H., M.A., *Vice-Pres.*, 1 York Road, Edgbaston	18*th Nov.*, 1889.
MURRAY, Miss MARION, 9 Hans Road, S.W.	3*rd Feb.*, 1896.
RASHDALL, Rev. HASTINGS, M.A., Balliol College, Oxford	8*th April*, 1889.
RHODES, GEORGE S., The Gables, Saltburn-by-the-Sea, Yorkshire	20*th Nov.*, 1893.
RITCHIE, Prof. D. G., M.A., *President*, The University, St. Andrews	16*th Nov.*, 1885.
ROBINSON, ARTHUR, M.A., King Henry VIII. School, Coventry	4*th March*, 1895.
ROGERS, H. L., B.A., St. John's School, Leatherhead	16*th Nov.*, 1896.
RUSSELL, Hon. B. A. W., M.A., 44 Grosvenor Road, S.W.	17*th Feb.*, 1896.
RYLE, R. J., M.A., M.D., 15 German Place, Brighton	31*st March*, 1890.
SCHWANN, Mrs., 4 Princes Gardens, S.W.	24*th May*, 1897.
SELBY-BIGGE, L. A., M.A., 43 Elvaston Place, S.W.	3*rd Dec.*, 1888.
SENIER, Prof. A., M.D., Ph.D., Queen's College, Galway	19*th April*, 1880.
SHAND, ALEXANDER F., M.A., *Vice-Pres.*, 1 Edwardes Place, Kensington, W.	25*th April*, 1892.
SHEBBEARE, Rev. CHARLES J., B.A., Milford, near Godalming	14*th Dec.*, 1891.
STOUT, G. F., M.A., *Editor*, The University, Aberdeen	21*st Nov.*, 1887.

	Date of Election.
STONEY, G. JOHNSTONE, M.A., D.Sc., F.R.S., 8 Upper Hornsey Rise, N.	17*th Dec.*, 1888.
STRONG, Rev. T. B., M.A., Christ Church, Oxford	17*th Dec.*, 1888.
STURGE, Miss Mary C., 8 Holly Place, Hampstead	25*th Feb.*, 1889.
STURT, HENRY, 4 Park Crescent, Oxford	20*th Nov.*, 1893.
SULLY, Prof. J., M.A., 10 Park Hill, Ealing, W.	8*th Feb.*, 1897.
VICAJEE, FRAMJEE R., Barrister-at-Law, High Court of Judicature, Bombay	22*nd Nov.*, 1886.
WEBB, CLEMENT C. J., M.A., Magdalen College, Oxford	2*nd Feb.*, 1890.
WENLEY, Prof. R. M., M.A., D.Sc., East Madison Street, Ann Arbor, Mich., U.S.A.	3*rd Feb.*, 1896.
WESTERMARCK, Prof. EDWARD, 18 Keppel Street, W.C.	29*th Nov.*, 1897.